ARROYO SECO

SCIENTIFIC AMERICAN SUPPLEMENT

Copyright 1917 by Munn & Co., Inc.

VOLUME LXXXIV]
NUMBER 2171

★ NEW YORK, AUGUST 11, 1917 ★

[10 CENTS A COPY
$5.00 A YEAR

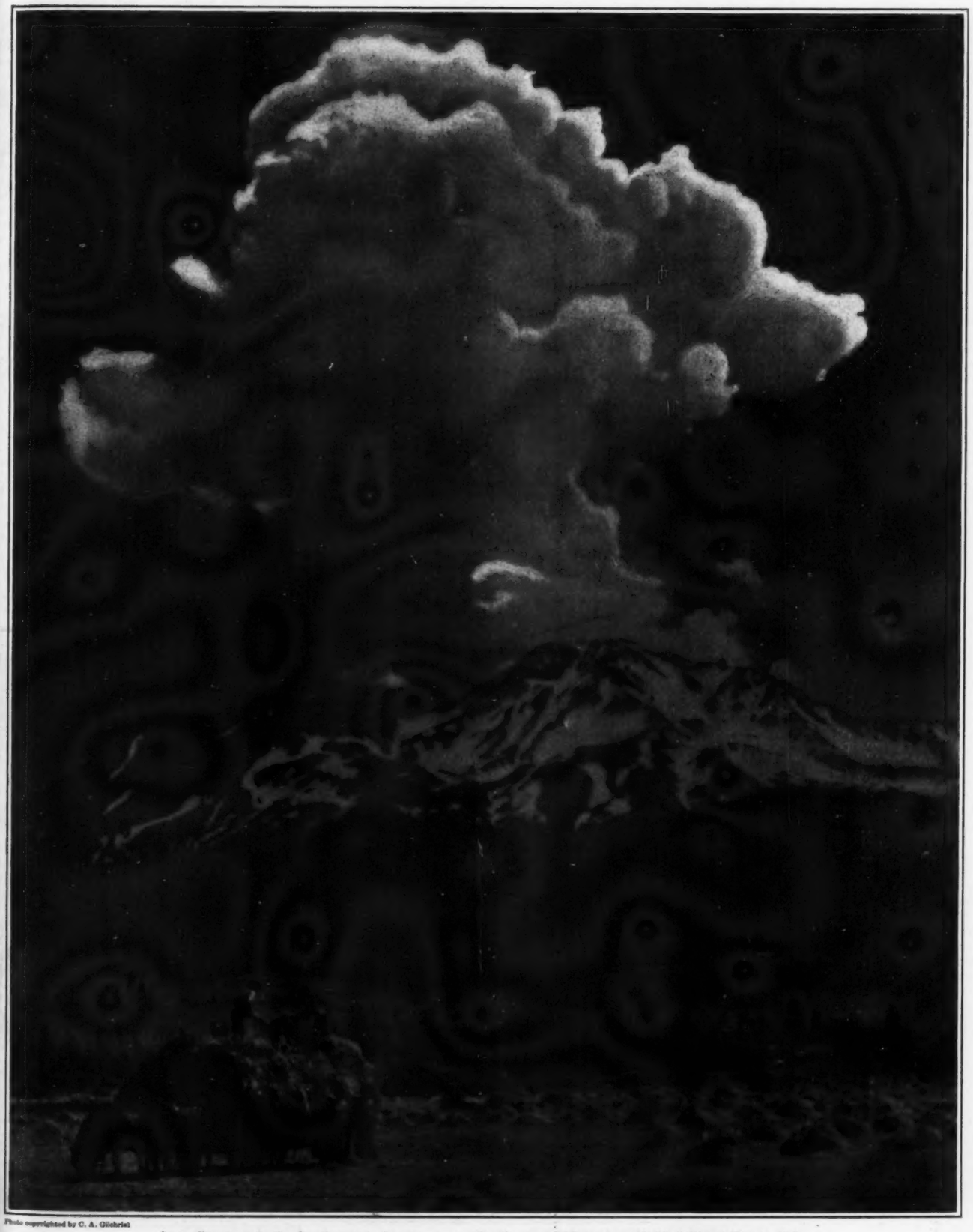


Photo copyrighted by C. A. Gilchrist

An earlier stage in the development of a very severe thunderstorm either over or behind Mount Shasta, Cal.

THE EFFECT OF THE WEATHER ON FOREST FIRES [See page 88]

The Nature of Matter—I.*

This is Not a Universe of Dead Atoms, But of Active Energy

By J. C. Whitehorn, Doane College, Crete, Neb.

FROM the time when men began to wonder at the world and ponder its problems they have puzzled over the nature of the universe. Sensing an external world of objects and feeling the vital necessity of dealing with these objects, they have sought for an understanding of the universe which would give them a mastery over it. And in their more contemplative moments they have tried to attain to a more comprehensive view which would not only be serviceable in handling objects but would also give a true presentation of the reality of the universe. Through the whole range of solutions offered one can find an ever present attempt to conceive the universe in some unitary and comprehensive manner. Some have looked upon it as a chance collection of dead atoms propelled by blind force; others as pure idea or thought. Between these two extremes there have been all sorts of gradations and compromises.

The unreflective attitude of the average man, which may be called the common sense view, is that there is a certain substance or matter back of the phenomena of nature. This primary matter has certain properties such as color, odor, taste, etc. The phenomena of nature consist of the activities of this matter. Common sense, then, postulates three planes or kinds of reality, (1) a substratum of matter or substance, (2) the properties inherent in this substance, and (3) the activities of this substance.

This naïve solution is, however, quite unsatisfactory. In the first place, it is practically impossible to draw a fast line between activities and properties. For example, luminosity was formerly thought to be a property of matter, now it is considered an activity of matter. Activities and properties merge into one another until we can find no satisfactory line of demarcation.

In the second place, no one ever saw, heard, felt, tasted or smelled "matter." All of our knowledge of objects is a knowledge of properties, not of a "matter" or "substance." An object, as known to us, is simply a self-subsistent organization of properties. The idea of a hard and changeless block of reality which serves as a support to these properties is a purely metaphysical assumption.

And yet, even though there is no logical necessity, we feel a psychological need for the idea of a matter or substance to which we can *fasten* the qualities, as it were. The basis for this psychological need can probably be found in those universal and stable properties, extension and mass. We see about us objects undergoing various transformations of color, odor, density, etc. Most of the properties seem to be variable and changing, and sometimes absent altogether. But there are certain properties which are universally present in all objects, under all conditions and through all changes. These universal properties are mass and extension. Their persistency and universality indicate a "something" which endures and is constant through the changes. Hence, there has unconsciously grown up in our thought a concept of a stable and universal substance or matter which lies back of and gives support to the varying properties.

If, however, we think soberly and consistently upon the matter we shall come to the conclusion that this is a fault of unreflective thought, which has seized upon the more stable and enduring properties as a support for the variable and changing. Extension and mass are, in fact, merely those properties which are more permanent and universal. There is no logically necessary implication or assumption of any material entity, as ordinarily understood.

Let no one think, however, that by this statement we deny the existence of external and objective reality. We are not trying to reduce the world to a phantasmagoria of subjective ideas. We are attempting to get rid of a troublesome metaphysical assumption as to the nature of this objective reality. *Our purpose is to ascertain as accurately as possible the true nature of reality, and more particularly of that phase of objective reality commonly known as matter.*

We shall strive to conduct this investigation in the genuine spirit of a seeker after truth and therefore we shall consider of primary importance what science has to say upon the subject. And science has much to say. Modern physical research has shed great light upon this problem. The strange and erratic behavior of cathode rays and of the emanations of radio-active substances has greatly modified our conceptions of matter.

We shall take up first a discussion of those phenomena which occur when electricity is conducted through rare gases. If an electric current of high potential be passed through an air-tight glass tube containing gas at atmospheric pressure, an ordinary spark will be produced, just as in the atmosphere. If, however, the gas be somewhat exhausted, so as to create a partial vacuum, the current, which need not now be of high potential, will pass readily through the tube. Its passage will be marked by curious phenomena. The negative electrode (cathode) will be covered by a luminous layer. Next to this is a dark space, the Crookes dark space. Beyond this is a luminous part called the negative glow, and beyond this glow another dark space, the Faraday dark space. Between this and the anode is the positive column of alternately dark and light spaces called striae.

If the gas be still further exhausted to a pressure of about .01 mm. the above phenomena disappear and a bright phosphorescence appears on the sides of the tube. This phosphorescence is produced by radiations of very minute particles issuing in straight lines fromthe cathode. If a solid body be placed inside the tube so as to intercept the rays, it will cast a well-defined shadow. If the rays be focused upon a body, it may be heated even to incandescence. But what is of much more significance, these particles can be deflected from their normally straight path by means of a magnet. This indicates that the particles are either themselves magnetic or are electrically charged so that their motion sets up lines of magnetic force.

Perrin and later J. J. Thomson have shown that the deflection by a magnet is due to a negative charge of electricity carried by the particles. If the end of a conductor, carefully insulated and protected, be placed *near* a pencil of cathode rays, an electrometer connected to this conductor will show very little effect. But if the rays be deflected so that the particles fall directly *upon* the conductor, the electrometer shows a considerable negative charge. Furthermore, if a pencil of cathode rays be passed between two parallel plates which are oppositely charged at a great difference of potential, the beam of rays will be deflected *towards* the *positive plate*. These experiments indicate that *the cathode particles carry a negative charge of electricity.*

This gives us a clue as to the method of finding the velocity of these rays. Measurements have shown that a moving charged particle produces a magnetic field which is equal to the field produced, per unit of its length, by a linear conductor carrying a current equal to $e\ V$, where e is the charge on the moving particle and V is its speed. [1]Hence if the speed of a cathode particle is V and its charge e, its magnetic field is $e\ V$. If a magnet be brought to bear with field intensity H upon the cathode particle, the particle will be deflected from its path by a force $He\ V$ (the product of the two magnetic forces) and will follow a curved path.

Suppose now that two parallel plates be placed on opposite sides of the pencil of rays and a high difference of potential established between them in such a way as to pull the charged particles in the opposite direction from that in which the magnet deflects them. The electric force acting on the particle will then be the product of the charge on the particle times the strength of the electric field. If the latter be X, the force will be eX. Now the magnetic and electric fields, which counteract each other, can be so adjusted that there will be no deflection, at which time the opposing forces are equal. That is, eX equals $He\ V$. Then V equals X/H. Now X and H can be measured and hence V can be calculated. By this method Thomson has found the velocity of cathode particles to be 2.8×10^9 cm. per sec., which is about one-tenth the velocity of light (29.99×10^9).[2] The value for V varies slightly with the potential in the tube, but is fairly constant.

If we remove the electric field and measure the deflection caused by the magnet, we can calculate the radius of the curve of deflection. The force producing this deflection is from the above discussion $He\ V$. The centrifugal force of the particle whose mass we shall call m, moving with speed V in a circle of radius r, is, by mechancis, mV^2/r. Action and reaction are equal, hence

$$He\ V = mV^2/r. \quad \therefore V/Hr = e/m.$$

Now H can be measured, r can be calculated and V can be determined as shown above. Hence the ratio e/m can be found. Thomson, by this method, obtained a value for e/m of 1.7×10^7.

The values of V and e/m have thus been shown to be interdependent. This indicates an intimate relationship between the velocity, the charge and the mass. Since V, e and m have been practically constant in the above mentioned experiments, we have been unable to determine the nature of their inter-relationsip. We must search for a case in which these values are variable in order to find their mutual relations.

Such a case has been found in the radium emanation. The phosphorescent action of the cathode rays led scientists to investigate phosphorescent substances. Investigation of uranium salts and pitch blende ores led to the discovery by Mme. Curie of the element radium. This substance seems to defy the laws of physics. It constantly gives off an enormous amount of energy without seeming to lose weight or be consumed. This energy is given off in the form of radiations. Ernest Rutherford has found that these radiations are composed of three distinct types of rays, called the alpha, beta and gamma rays. Becquerel showed that the beta rays behave in every respect like the cathode rays. They are composed of negatively charged particles having the same relations between mass, speed and charge as do the cathode particles. Becquerel, by practically the same method as Thomson used for cathode particles, determined the velocity and the ratio e/m for the beta rays. The value which he found for e/m does not differ much from the value of e/m found by Thomson for the cathode particles (1.7×10^7). He observed, however, that the velocities varied from 6×10^9 to 2.8×10^{10} cm. per sec.

This variation in velocity led Kaufmann to make more accurate determination of e/m to find whether the value of this ratio varied with the velocity. He made the very important discovery that *e/m decreases when the speed increases.*[3] If we assume that the charge e is constant, then *the mass* of the beta particle *appears to increase with increase of velocity.* This is not the old law of mechanics that the *momentum* varies with the speed, but a new discovery that the *mass itself varies with the speed.* Now it seems to be quite well established at least for bodies of appreciable size, that the mass of an *uncharged* particle does not vary with the velocity. This renders it probable that the variation of the mass of a beta particle with the velocity is due to the electric charge.

Indeed, if we stop to reflect, we can find other considerations which would lead us to the conclusion that an electric charge on a moving body will affect its mass. Suppose that a certain force F is applied to an uncharged particle, imparting to it a certain acceleration a. If now the same particle be *charged*, the force F will be unable to impart to it the same acceleration, because the force not only has to overcome the same inertia as before, but also, by moving the charged particle, sets up lines of magnetic force. By the law of the conservation of energy, this force cannot be created from nothing. Additional force is required to set up these magnetic lines. Hence more force will be required to produce the same acceleration. Now by Newton's second law F equals ma. Then m equals F/a. Hence in the case of the uncharged particle considered above, m equals F/a; but in the case of the charged particle m equals $(F+f)/a$. In other words, the *mass of the particle has been increased by the electric charge which it carries.* The increase of mass depends upon the magnetic field set up and this depends, in turn, upon the amount of electric charge and the speed of the moving particle. Therefore the mass of a particle carrying a constant charge is a function of the velocity. It must furthermore be remembered that, when magnetic lines have once been set up, they tend to keep the particle in motion. Hence in either case, whether of negative or positive acceleration, an electric charge on a particle increases its mass.

These theoretical considerations (which, it must be remembered, deal only with moving particles) bear out the empirical findings of Kaufmann. We can therefore conclude that the mass of the beta particle is due, *in part at least*, to the moving charge. And if in part, then why not altogether? There is scientific material which seems to indicate that *the whole mass* is due to the charge and its velocity.

J. J. Thomson has worked out mathematical formulae for the mass of a moving charged particle. He has shown that the part of the mass due to the moving charge (the "electrical" mass, as it were,) is $2e^2/3a$, where e is the charge, a the radius of the particle and ϵ a variable depending upon the velocity. This variable ϵ is taken as unity when V is very small compared to the velocity of light, and becomes larger as V approaches the

*Republished from *Popular Astronomy.*

[1]Duff-Carman, p. 387.

[2]Duff-McClung, p. 495.

[3]Duff-McClung, p. 507.

velocity of light. The variable a is the ratio of the mass at any given speed to the mass at a certain slow speed (taken as unity). Thomson has calculated the value of a at different velocities. The results are given in the following table, where they are compared with certain experimental results of Kaufmann. The latter has worked out experimentally the values of e/m at different velocities. With these results and taking the mass at a certain slow speed as unity, he has calculated the ratios of the mass at different speeds to this unit mass. His experimental results for the whole mass agree with Thomson's theoretical calculations for the electrical mass within the limits of experimental error, as shown by the following table:[4]

Velocity in cm per second	KAUFMANN	THOMPSON
	Ratio of *whole* mass at given velocity to *whole* mass at slow velocity (taken as 1)	a the ratio of *elec.* mass at given velocity to *elec.* mass at slow velocity (taken as 1)
2.85×10^{9}	3.09	3.1
2.72×10^{10}	2.43	2.42
2.59×10^{10}	2.04	2.0
2.48×10^{10}	1.83	1.66
2.36×10^{10}	1.65	1.5

This close agreement leads us to conclude that *the electrical charge and its motion are sufficient to account for all the mass of the particle.* Mass is wholly a product of electrical charge and motion.

The objection will instantly be raised that such a conclusion is untenable, for all are familiar with the fact that uncharged bodies have mass. But an uncharged body is a rather difficult thing to define. It is, in the first place, merely a *relative* distinction. When we speak of a body as "uncharged" we mean that there is no difference in electrical potential between it and the earth. In the second place the term "uncharged," when applied to a body, means simply that the body, *as a system*, is at the same potential as the earth. It does not mean that there is no electricity in the body, but that what charges of electricity there may be are *balanced* so that the system does not show them. Thus copper sulphate may be dissolved and dissociated in water. Under such conditions it most certainly shows evidences of the presence of electrical charges. But these charges are of opposite kinds and are present in such amounts as to exactly balance so that when the $CuSO_4$ is precipitated the one kind of electricity exactly balances the other and the precipitate gives no evidence of their presence. We say that the precipitate is uncharged, whereas in fact the particles of which the precipitate is composed are fairly alive with charges which will reappear if the copper sulphate is again dissociated. Thus we see that a body might be entirely made up, in its inner constitution, of disembodied charges moving about with great velocity, while the body as a whole showed no signs of this electricity and hence would be "uncharged." If these disembodied charges were continually moving at uniform speed, their masses and consequently the aggregate mass of the whole body would be constant, and would not be affected by movements of the body as a system. Thus the fact that uncharged bodies have mass does not invalidate the hypothesis that mass is due to electric charges in motion, for the mass of such an "uncharged" body may be due to the rapid movement of *internal* charges which are *equally balanced* and hence do not give any charge to the body as a system.

From the consideration brought forward above, showing that mass varies with velocity, that an electric charge in motion can produce mass, and that these factors of charge and motion are sufficient to account for all the mass, we are led to the conclusion that mass is not a primary but a secondary quality and is derived from an electric charge in motion. We are now in a position to make the hypothesis that *matter is composed altogether of electrical charges*, which, by their motion, give to bodies their mass.

This hypothesis has been worked out quite in detail by Thomson, Rutherford and others, and it serves so well to explain the phenomena of physics and chemistry that it is coming to be generally accepted. It is called the corpuscular or electron theory.

I cannot hope within the limits of this paper to undertake a comprehensive discussion. I will have to leave untouched a great mass of interesting facts and a whole body of new theory concerning the bearing of this new idea upon such phenomena as heat, polarization, electrical and thermal conduction, incandescence, etc. I will attempt only to indicate the general outlines of the theory and to point out its application to a few phenomena.

The beta particles and the cathode particles, which are identical in their properties, are taken to be the unit of electrical charge and are called *electrons* or *corpuscles*. The atom is thought to be made up of corpuscles. But corpuscles are negative charges and hence repel each other. There must therefore be a *positive charge* which holds them together in the atom. The form in which this positive charge occurs in the atom is at present a matter about which we have very little information.

Thomson, in his calculations, considered that the positive charge was spread with uniform density through a sphere of quite large radius, within which the corpuscles were arranged.[5] Rutherford has shown, however, that the distribution of positive electricity assumed in the Thomson atom was far too diffuse to account for many facts which have later been discovered. According to this view of Rutherford's, the positive electricity is concentrated in a very minute nucleus which is thought to have a radius of 1/1800 of that of the negative electron.[6] But this minor disagreement need not divert us from the main consideration, or the difference between these two views is merely a refined and technical distinction. Rutherford's view is more in accordance with observed facts but in order to present Thomson's work as originally developed we will adhere to his theory of a sphere of positive electrification of uniform density.

The positive electricity attracts the corpuscles toward the center of this positive sphere, while their mutual repulsion drives them away from it; when in equilibrium they will be distributed in such a way that the attraction to the positive electrification is just balanced by the mutual repulsion of the corpuscles. When there is only one corpuscle it will of course go to the center of positive electrification; two corpuscles will be in equilibrium on opposite sides of and at equal distances from this center; three, at the corners of an equilaterial triangle; four, at the corners of a regular tetrahedron; and six, at the corners of a regular octahedron, the center of each of these configurations being at the center of the positive sphere.

Thomson has been unable to work out the arrangement of more than six corpuscles in three dimensions. He has, however, solved the case where the corpuscles are confined to a plane passing through the center of the positive sphere. According to his calculations, stable equilibrium is assured when the corpuscles are arranged in concentric rings as follows: for numbers of corpuscles from 2 to 5, one ring; from 6 to 10, two rings, 5 to 11 in the outer ring; from 17 to 31, three rings, 11 to 15 in the outer ring; from 32 to 48, four rings, 15 to 17 in the outer ring; from 49 to 69, five rings, 17 to 21 in the outer ring; from 70 to 93, six rings, 21 to 24 in the outer ring. A complete table is given on pages 109-110 of Thomson's "The Corpuscular Theory of Matter" showing the number of rings and the number of corpuscles in each ring for groups of corpuscles ranging in number from 1 to 100

Let us take for example the arrangement of corpuscles in those configurations which have 20 in the outer ring. The following table gives the number in each ring:[7]

Ring	A	B	C	D	E	F	G	H	I
5th (outer)	20	20	20	20	20	20	20	20	20
4th	16	16	16	17	17	17	17	17	17
3rd	13	13	13	13	13	13	14	14	15
2nd	8	8	9	9	10	10	10	10	10
1st (inner)	2	3	3	3	3	4	4	5	5
Totals	59	60	61	62	63	64	65	66	67

The group of 59 corpuscles shown in Column A has 20 in the outer ring, 16 in the next, 13 in the next, 8 in the next and 2 in the inner ring. Suppose that the charges in this group balance, *i.e.*, that the positive charge equals the sum of the negative charges of the corpuscles. If an additional corpuscle were shot into the group, the 60 corpuscles would then arrange themselves with 20 in the outer ring, 16 in the next, 13 in the next, 8 in the next and 3 in the inner, as in Column B. Since the positive and negative charges at first balanced, this new group in Column B has one excess negative charge. If two corpuscles were shot in, the group would then have two excess negative charges, and the 61 corpuscles would be arranged as shown in Column C. If three were shot in, the group would have three excess negative charges and the 62 corpuscles would be arranged as shown in Column D. Similarly, if 4, 5, 6, 7 or 8 corpuscles were shot in, the groups would have 4, 5, 6, 7, 8 excess negative charges and the 63, 64, 65, 66 or 67 corpuscles would arrange themselves as in Columns E, F, G, H and I. These arrangements would all be stable. If, however, more than 8 corpuscles were shot in, the rearrangement would put 21 in the outer ring, and so would pass outside the group we are considering. Moreover, it would be loading a disproportionate number of corpuscles upon the comparatively small positive charge, which it must be remembered is the same as in Column A. These arrangements then would tend to eject corpuscles and come back to one of the forms with 20 in the outer ring. The stable groups then have 59, 60, 61, 62, 63, 64, 65, 66 and 67 corpuscles and have respectively 0, 1, 2, 3, 4, 5, 6, 7 and 8 excess negative charges.

If, however, instead of starting with an evenly balanced group of 59 corpuscles, we had started with the group of 67 corpuscles with a positive charge sufficient to balance them and, instead of shooting corpuscles in, had taken them away, the resulting groups would have had 66, 65, 64, 63, 62, 61, 60 and 59 corpuscles, respectively, and would have had excess *positive* charges of 0, 1, 2, 3, 4, 5, 6, 7, 8.

The group having an excess positive charge of, say, 2 would attract to itself either two groups with an excess negative charge of one each or one group with an excess negative charge of two. That is, the number of excess positive charges measures the attraction for negative groups, and hence may be considered the electro-negative valency. Similarly the number of excess negative charges may be considered the electro-positive valency. We can now give a table showing these results:

Group Letter	A	B	C	D	E	F	G	H	I
No. of Corpuscles	59	60	61	62	63	64	65	66	67
Electro-positive valency obtained by shooting electrons into 59 (A)	0	1	2	3	4	5	6	7	(8)
Electro-negative valency obtained by taking electrons from 67 (I)	(8)	7	6	5	4	3	2	1	0

These groups are arranged according to the number of corpuscles they contain. The valency is seen to be a function of the number of corpuscles. Now, since we have shown that mass is due to electric charges, if there actually exist atoms analogous to these groups, the number of corpuscles of which they are composed would determine their atomic weights. Hence the valency, being a function of the number of electrons, would be a function of the atomic weight. The corpuscular theory then, as worked out by Thomson, leads to the conclusion that valency is a function of atomic weight.

If we were to take up a consideration of those groups of corpuscles having 21 in the outer ring we could in like manner show that the valencies of this group would run from 1 to 8, with an additional group with valency 0 (signifying an inert group) at each end. This would indicate that valency is not only a function of the atomic weight but also a periodic function.

This theoretical deduction from the corpuscular hypothesis is amply confirmed by empirical science. The periodic tables of the elements arranged by Mendeleeff and Meyer show clearly that not only valency but many other properties as well are periodic functions of the atomic weights. The following short table illustrates this fact, with reference to valency:

El.	He	Li	Gl	B	C	N	O	F	Ne	Na	Mg	Al	Si	P	S	Cl	Arg
At. wt.	4	7	9.1	11	12	14	16	19	20	23	24.4	27	28	31	32	33.5	39.9
+Val.	0	1	2	3	4	5	6	7	0	1	2	3	4	5	6	7	0
−Val.	0	7	6	5	4	3	2	1	0	7?	6?	5	4	3	2	1	0

One can ask for no more beautiful confirmation of any hypothesis than this confirmation of the corpuscular theory by the Periodic Table of the elements.

This confirmation is still more remarkable when we realize that Thomson's calculations have all been based upon the arrangement of corpuscles in a plane. It is to be hoped that three-dimensional arrangements will soon be calculated. If this can be accomplished it will probably give us a still clearer insight into the inner constitution of the atom.

If we return to a consideration of Thomson's groups we shall find that, according to his hypothesis, the atom of one element could be changed into the atom of another element by the loss or gain of one or more corpuscles. This would be equivalent to a transformation of matter. A few years ago such a theory would have been scoffed at and ridiculed. But here again empirical investigation has supported the theory. Ernest Rutherford, through a study of radio-active substances, has found that such transformations are actually taking place.[8]

Radium, as mentioned earlier in this paper, is continually giving off radiations. These radiations are of three kinds, alpha, beta and gamma. The first kind of radiations, the alpha rays, are composed of alpha particles. The alpha particle is very heavy, having four times the atomic weight of hydrogen. It carries two unit positive charges. It is thought to be an atom of helium, carrying twice the ionic charge of hydrogen. The alpha particles furnish the positive electrification for the atom. The beta rays are composed of beta particles, which are simply corpuscles or electrons. The sudden emission of the beta particle or electron from the atom produces an electro-magnetic disturbance in the ether, just as the sudden stopping of a cathode particle produces an X-ray. These electro-magnetic pulses produced by the emission of beta particles are called gamma rays and behave exactly as X-rays.

[TO BE CONTINUED]

[4] Thomson, p. 33.

[5] Thomson, p. 103.

[6] Rutherford, pp. 129-130.

[7] Thomson, p. 109.

[8] Duff-McClung, pp. 506-510.

Muscular Power and Endurance of the Human Body

As Revealed by the Mechanics of Motion

By Armin Elmendorf,* M.Sc.

In view of the increasing demand for information concerning the endurance of men in violent exercise of short duration, the author recently conducted experiments to see whether such a physical quality could be represented graphically so that it could be studied. Exercises were selected that bring into action practically the entire body, such as running up a steep incline, swimming, running on a level track, and by weight lifting. The first of these exercises would be represented in military functions, for example, by a charge and the last would be quite analogous to the work of "digging in." No attempt was made to determine the amount of work men can do in long periods of time such as in the eight-hour day, nor was it the purpose to estimate human power in terms of the unit, the horse-power, when effort was controlled and extended over several hours of time. The investigation was limited to a study of endurance when working continuously at the very peak of exertion as in a race or in various military operations.

To explain the procedure adopted and to bring out

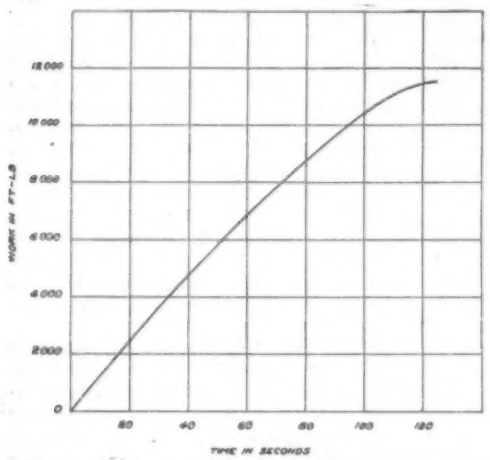
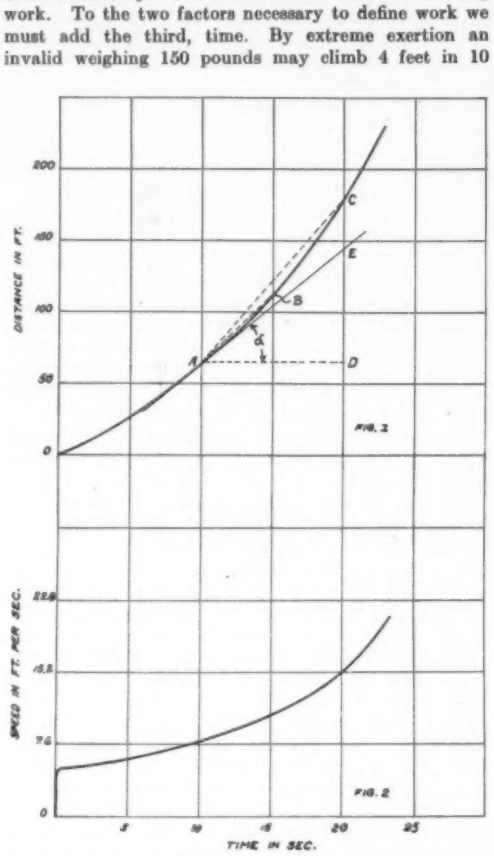


Fig. 4. Work-time curve for person raising a 32.5 lb. weight from the floor to the height of reach

the significance of the results it is necessary first to outline in some detail the exact meaning of the terms work, power, energy, and speed, and to differentiate between them. First in regard to the use of work it must be thoroughly understood that the operation it expresses is independent and absolutely unconcerned with time. A host of ants marshalled into harness would by working many weeks do exactly the same amount of work in carrying a half dozen sacks of wheat, grain by grain, up to the farmer's attic as the farmer himself carrying a sack at a time. The science of mechanics defines work as the product of force into distance. This definition does not bring in the notion of time. Inasmuch as work is the product of two factors its unit of measure must indicate these factors. Thus a boy drawing his wagon up hill may exert a pull of 20 pounds. When he has pulled the wagon through a distance of 10 feet he has done 200 *foot-pounds* of work. A man weighing 150 pounds climbing a ladder has done 600 foot-pounds of work when he has ascended 4 feet, irrespective of whether it took him a few seconds to do this or as many hours.

While the use of the term *work* demands some explanation, this is till more imperative in regard to *power* and *energy*. Maurer's Technical Mechanics recognizes this need in showing the variety of uses to which the latter words are put. "In common parlance the word power has many meanings. Thus we hear of the power of a giant, power of example, power of the press, etc.; and of things mechanical, we hear such expressions as a powerful derrick, a powerful cannon, a powerful pump, etc. On reflection we note that the adjective in these three expressions probably does not refer to the same feature of the derrick, cannon and pump. A derrick is probably called powerful because it can lift a very heavy body, or exert a great lifting *force*. A cannon is generally called powerful because it can project a heavy shot with great velocity. A pump is probably called powerful because it can elevate or transport a large quantity of liquid in a short space of time, or perform much *work per unit time*." The word is here used to express three distinct ideas, in the case of the derrick it means force, in the cannon it denotes energy, and in the pump, rate of doing work. Technical terminology restricts its use to the last idea entirely. Power is defined as the rate of doing work. To the two factors necessary to define work we must add the third, time. By extreme exertion an invalid weighing 150 pounds may climb 4 feet in 10



Figs. 1 and 2. Distance-time and speed-time curves for a person assumed to be running on a horizontal track

seconds. His power would then be $\frac{150\times4}{10}=60$ foot-pounds per second. A normal man of the same weight by maximum effort may be able to do the 4 feet in 3 seconds. His power would be $\frac{150\times4}{3}=200$ foot-pounds per second. The unit of power here used is the foot-pound per second. A larger unit is the horse-power

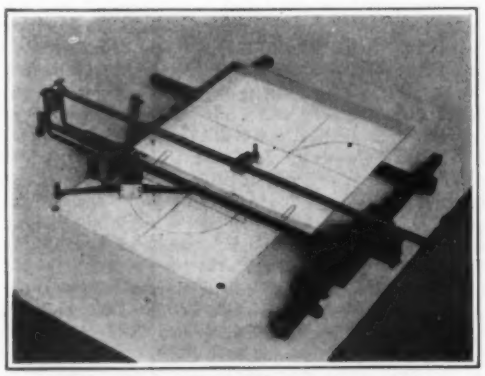


Fig. 3. The author's differentiating machine

Fig. 6. Fatigue curve traced by an ergograph, showing effect of exercise in increasing the force of the muscles

which is approximately the power of a common horse working a day and is defined as the power of the machine which does 550 foot-pounds of work in one second. The normal man climbing the ladder has $200\div550=0.364$ horse-power for the short period of 3 seconds.

As an interesting diversion to determine the horse-power a normal person exerts in ascending stairs at a rapid pace, the author conducted a few trials with persons of varying weight running upstairs from the first to the fourth floor of an office building. The time required was on the average about 14 seconds and the power for this time interval ranged from .7 to 1.2 horse-power for the different individuals.

The definition of power introduced the idea of rate, power is the time rate of doing work. Velocity, or speed when direction is of no importance, is spoken of as the time rate of displacement. It will be necessary to distinguish average speed from instantaneous speed. Thus if a boy in running covers 10 feet in the first second

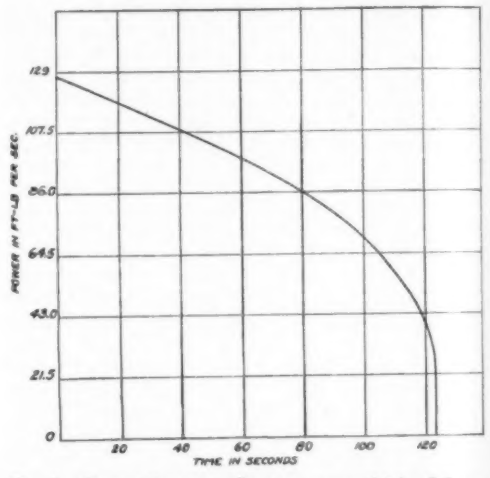


Fig. 5. Power-time or endurance curve obtained from the work-time curve of Fig. 4 by differentiation

his average speed is 10 feet per second for the first second. If he covers 12 feet in the next second his average speed for the second interval is 12 feet per second and his average speed for the first two seconds is $\frac{10+12}{2}=11$ feet per second. If he covers 14 feet in the third second his average speed during the third second is 14 feet per second and his average speed for the first three seconds is $\frac{10+12+14}{3}=12$ feet per second, etc. The boy's speed is changing continuously as he runs, and the question naturally arises how far would the boy run during the fourth second if at the end of the third second his speed remained constant; in other words what is his instantaneous speed at the end of three seconds?

Let us attempt to find a runner's instantaneous speed at the end of the tenth second. We will need some data for this purpose which may be readily obtained. By placing persons with stop watches at uniform intervals along a running track, say at 20-foot stations and starting all watches at the instant the gun is fired and the run is begun, the time to reach each station will be obtained. This may be plotted against the distance. Such a graph is shown in Fig. 1. From this curve it will be seen that at the end of 10 seconds the distance covered is 65 feet and at the end of 20 seconds the distance is 180 feet, so that the average speed for this ten second interval is $(180—65)\div10=11.5$ feet per second. It is represented geometrically in the triangle ACD by the ratio of CD to AD. In other words the average speed is given by the tangent of the angle CAD. Similarly the average speed for the interval from 10 to 15 seconds is given by the tangent of the angle BAD and is here $\frac{110—65}{5}=9$ feet per second. Now as the time interval is taken shorter and shorter the point B approaches A and the chord AB approaches as a limiting position the tangent to the curve at A while the angle BAD approaches the angle the tangent at A makes with the horizontal, that is the angle $\propto$. But the average speed is measured by the angle the chord makes with the horizontal, and since the chord approaches the tangent as a limit as the

*Instructor in Mechanics, University of Wisconsin, Madison.

time increment becomes indefinitely small, the angle between the tangent and the horizontal measures the limiting speed, that is the speed at the instant when ten seconds have elapsed. This means that if the runner has kept up exactly the same speed for the next ten seconds as he had at the end of the tenth second the distance he would cover during that interval would be given by DE.

To proceed from the given distance-time curve to the instantaneous speed-time curve shown in Fig. 2 is a simple matter. At the ten second division on the horizontal line we will erect to some scale the value of the tangent of the angle at $\propto$ A, at the 15 second division the value of the tangent at the angle $\propto$ at B, etc. Connecting these points gives the smooth speed-time curve of Fig. 2. While this is a simple operation the labor involved is quite large. The author's differentiating machine of Fig. 3 performs the entire operation mechanically, thereby saving a great deal of time. It has for its function the plotting of the tangents without measuring the angle, performing the operation of the calculus known as differentiation.[1]

One step further takes us from the speed-time curve to the power curve, for by simply multiplying velocity by force we get power. Thus if a man raises his weight against the force of gravity at a certain speed as in climbing a ladder or in running up stairs, he exerts

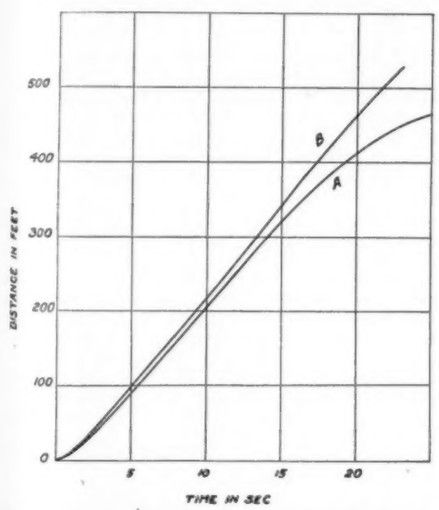


Fig. 7. Distance-time curves for men running up-hill a distance of 450 ft. A is the curve of an untrained man but a good runner, and B is the curve of an athlete

power; and the power a man has rather than the instantaneous force he can exert measures his effectiveness in action, that is his endurance.

To determine the endurance of the human body for work demanding primarily the use of the back, shoulder, and arm muscles an experienced weight lifter was asked to raise a 32.5 pound weight from the floor to his height of reach. The distance through which the weight was raised multiplied by 32.5 gave the work done in each lift. The time for each five lifts was read, giving all the data the work-time curve of Fig. 4. Operating upon this curve with the differentiating machine gave the curve of Fig. 5 which may be called the endurance or power curve for this particular individual in weight raising. Different weights would of course give curves varying both in height and length. The points to be noted in Fig. 5 are first that the maximum power is obtained at the start, as may be expected, and that the power decreases uniformly with time up to the end of two minutes, when the expenditure of energy had reached the point at which it was almost impossible to raise the weight another time, and being completely exhausted the man "gave up." It must be borne in mind that the weight lifting was carried on with a maximum effort throughout the test. The initial power of 127 feet-pounds per second or 0.23 horse-power is the effective or useful power, namely that of lifting the weight. The internal power, that is the power expended in lowering the weight and in raising and lowering the body should of course also go to the person's credit but it is so indeterminate that it was not considered here. Probably if these expenditures were known the maximum power developed would have been very near one-half horse-power.

The remarkable similarity between the power curve and the ordinary fatigue tracing should be noted. Fig. 6 shows a tracing obtained by Professor Aducco[2] to bring out the effect of training on the strength of muscles. The tracing was made by ergograph, an instrument recording the range through which a weight is lifted by a finger. The curve is in reality also a power curve. Since the abscissae represent time and the ordinates are equally spaced, each ordinate stands for the work done during the same time interval, that is power. Like the fatigue tracing, successive power curves will indicate

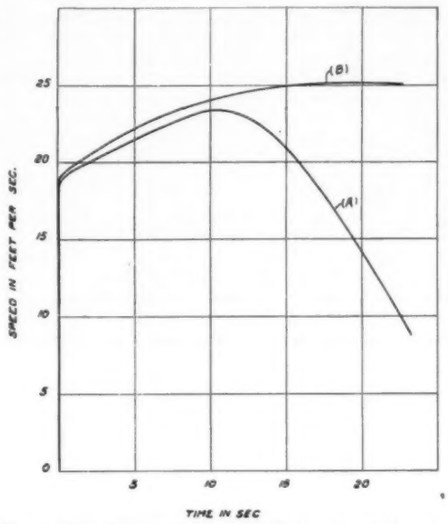


Fig. 8. Speed-time curves obtained from the distance-time curves of Fig. 7 by differentiation

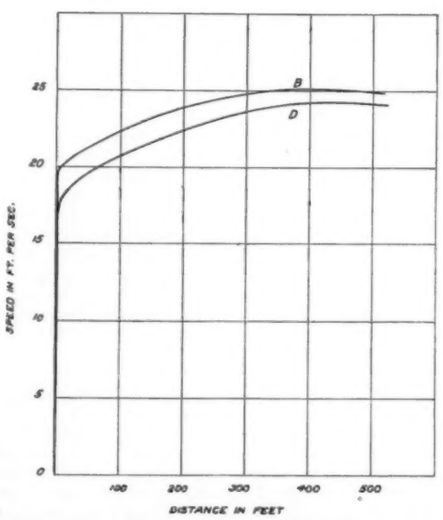


Fig. 9. Speed-distance or endurance curves for trained men running up-hill

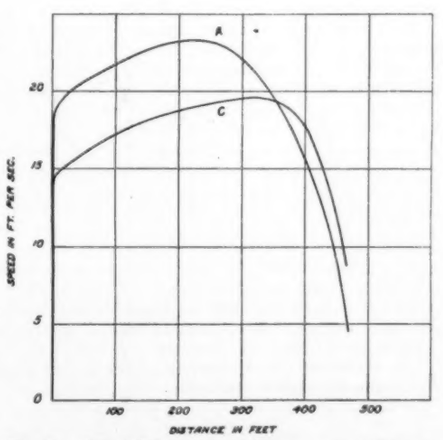


Fig. 10. Speed-distance or endurance curves for untrained men running up-hill

improvement in endurance. A study of power curves taken at various times while a person is under training should present a fertile field of investigation, one that would be of great interest and importance to athletic directors and officers in charge of military training camps.

As a second measure of endurance, tests were made on men running up hill. Fifty foot intervals were marked off on a walk having a uniform grade of 10 per cent, and recorders with stop watches were stationed at each division. At a given signal all the watches were started and the runner left his mark, the time being taken at the instant the runner passed each of the stations. This data plotted gave curves of which the two in Fig. 7 are representative. A is the curve of an untrained man and B that of an athlete. Inasmuch as the walk upon which the test was conducted had an uphill grade of 10 per cent, each 100 feet passed over represents the work of lifting one's weight vertically through a distance of 10 feet. Now if the runner reaches a maximum speed of 25 feet per second he raises his weight 2.5 feet per second so that if he weighs 150 pounds his power for that speed is $\frac{150\times 2.5}{550} = 0.68$ horse-power. Multiplying velocity in the vertical direction by weight gives the power in foot-pounds per second and dividing by 550 gives it in horse-power. Here again, this is not total power but

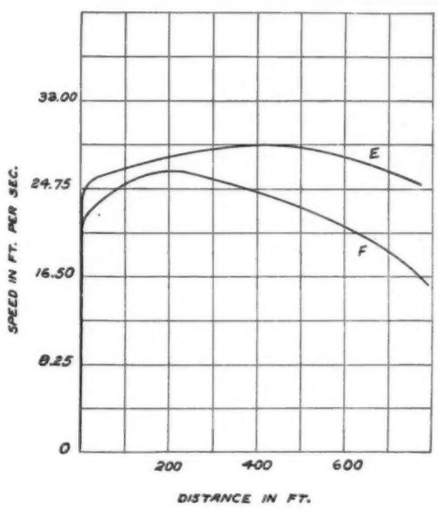


Fig. 11. Speed-distance or endurance curves for two men running on a horizontal track a distance of 800 feet

only what may be called external power, namely that of working against the external force of gravity. To obtain the total power we would have to add that lost internal work such as body friction, radiation, etc. By analogy it would be called internal power. Comparing the human body as a machine with the steam engine we might say that total power would correspond to indicated power in the engine, internal to friction power, and external to brake power.

The curves of Fig. 8 are the result of operating upon the distance-time curves of Fig. 7 with the differentiating machine. While the ordinates represent speed, they also represent power to some other scale as explained. Figure 8 then is a graphical record of endurance in running uphill. Two points are significant, first that the maximum power is not attained at the start as was the case in weight lifting, but in the interval from ten to twenty seconds after the start, second, that the peak once attained the power of the untrained man falls very rapidly while the trained man loses power only gradually. In a charge the soldier must possess enough energy after having reached his destination to execute his orders of attacking or "digging in." His capacity of doing this is measured by the ordinate to the power curve at the time corresponding to his supposed arrival at the destination. The psychological element enters of course, but it is not unreasonable to assume that courage is as much determined by power as power is the result of courage.

Figures 9 and 10 show the variation of power with distance. The curves were obtained by finding the speed at certain instants from the speed-time curve and the distance at the corresponding instants from the distance-time curve. B and D of Fig. 9 are the curves of trained runners, A and C of Fig. 10 the curves of untrained men. The effect of training is so obvious that explanation is unnecessary. We can readily imagine the difference in execution of which B and D are capable

[1] A description of the differentiating machine will be found in the SCIENTIFIC AMERICAN SUPPLEMENT of February 12, 1916, and The *American Mathematical Monthly*, October, 1916.

[2] Fatigue by A. Mosso, translation by Margaret Drummond and W. B. Drummond, page 95

after having run 450 feet in comparison with A and C. The ideal time for action would correspond, of course, to that of maximum power, for it is at that time that the blood circulation and muscle stimulus have reached the intensity for the best effort of which the individual is capable.

In Fig. 11 are given the speed-distance curves of two men running on a horizontal circular track over a distance of 800 feet. Here again the maximum speed of the trained runner E is both greater and occurs at a later time than that of F, an untrained man, and the falling off power which is necessarily measured by speed alone is more rapid in the latter curve. In such exercise the body remains on the same level, hence no external work is done and all the energy is consumed in internal work, that is power in running in a horizontal track is entirely internal.

The endurance curves of Fig. 12 are for three men swimming various distances. They all have one characteristic in common, namely that the maximum speed or power is attained at the very start as is the case in weight lifting. The curve A is for a 100 yard dash man, B for a 200 yard swim, and C the curve of an untrained swimmer trying his time on a 60-yard swim. The preceding curves, namely the distance-time and speed-time curves were omitted because they showed nothing further than is shown in the speed distance curves of the figure. The marked dip in B's curve at 135 yards indicates a slowing up for a short distance, possibly to catch the "second wind." A swam a crawl stroke and B and C each swam the breast stroke.

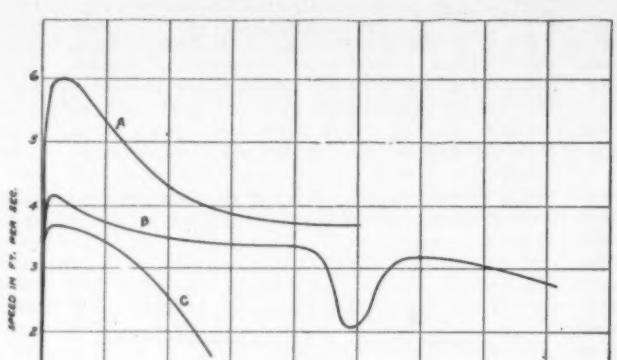


Fig. 12. Endurance curves for men swimming

While the speed distance curves give a good graphical representation of endurance or power, the total muscular power expended cannot be accurately determined, not even in running up stairs although here most of the energy goes into raising the body against the force of gravity and thus may be measured. It seems reasonable to assume that since the maximum speed in running up hill is 25 feet per second and the speed for running on a horizontal track is only 4 feet per second more, and the power expended by a normal person in raising his weight when doing 25 feet per second up hill on a 10 per cent grade is about three-fourths horse-power, the total power in running up hill must be considerably more. A retardation of 4 feet per second enables the man to raise his weight vertically 2.5 feet per second, that is, the retardation corresponds to ¾ horse-power, so that it is not at all improbable that a good runner doing a 220 yard dash exerts himself during the run to the extent of 1½ horse-power.

Explorations in the Hawaiian Islands

During last summer, Prof. A. S. Hitchcock, custodian of the section of grasses of the division of plants, U. S. National Museum, assisted by his son, A. E. Hitchcock, traveled in the Hawaiian Islands studying and collecting the flora, especially the grasses, making what might be termed a forage survey.

The islands visited were Kauai, Oahu, Lanai, Molokai, Maui, and Hawaii, these comprising all the islands of the main group except the two small ones Kahoolawe and Niihau. These islands are all of volcanic origin and composed of lava, except a very small part which is of coral formation. Kauai, geologically the oldest island, shows the greatest effect of erosion, its deep canyons rivaling the beauty of the Grand Canyon of Colorado. The rainfall on the mountains of the windward side is excessive, that of Waialeale, the highest peak of Kauai, being as much as 600 inches per annum. On the contrary, the lee side of the islands is arid, the rainfall being often reduced to less than fifteen inches per annum.

To the south the islands are successively younger, Hawaii, the largest, being even now in a state of volcanic activity. On this island are situated the two highest peaks of the group, Mauna Kea, 13,825 feet, and Mauna Loa, 13,675 feet in height. There is scarcely any vegetation upon these peaks, above 10,000 feet, especially Mauna Loa, which is made up of comparatively recent lava. Much snow covers the peaks in winter, extensive banks persisting throughout the year. The magnitude of the mountain mass is greater than at first appears, because the cones arise from the very floor of the ocean, 18,000 feet below the surface, thus making the total height over 30,000 feet. So gradual is the slope from the sea to the summit, that the eye is deceived and the great height is not at first fully appreciated. The active volcano Kilauea (4,000 feet) with its pit of boiling lava, is on Hawaii, while Healeakala, said to be the largest crater in the world, is on Maui, the second largest island of the group.

Important agricultural industries of the island include sugar, live stock, and pineapples. The native Hawaiian population is decreasing, and it is only in the less accessible parts of the islands that the primitive customs still prevail. Here may be found the native grass huts made of a wooden frame-work filled in with a thatch of grass. The grass used for this purpose is usually pili, an indigenous grass abundant upon the rocky soil of the lowlands.

The cultivated trees and shrubs are of great variety and beauty, and are drawn from all tropical and subtropical land. The introduced flora is very pronounced in the region of the towns, ranches, and plantations, and one must go several miles from Honolulu to find indigenous or native plants. Of 60 species of grasses found on Oahu about fifty were introduced from foreign countries. One of the introduced trees of great economic importance is the algaroba tree, or kiawe, as the Hawaiians call it. It is found in a belt on the lowlands along the shores of all the islands and occupies the soil almost to the exclusion of other plants. The pods are very nutritious and are eagerly eaten by all kinds of stock. Its flowers furnish an excellent quality of honey. The Molokai ranch alone produces 150 to 200 tons of strained honey per year. The prickly pear cactus (a species going under the name of *Opuntia tuna*) has become extensively naturalized in the dryer portions of all the islands. Ranchmen utilize this for feed when other kinds become scarce, the cattle eating the succulent joints in spite of the thorns. Two introduced shrubs now occupying extensive areas have become great pests. These are guava, whose fruit furnishes the delicious guava jelly, and lantana, with clusters of handsome parti-colored flowers. In the moister portions of the islands large areas have been occupied by Hilo grass which has little value as a forage plant. The kukui or candlenut tree with its light, almost silvery, green foliage is now a common and rather striking element in the valleys and gorges.

The indigenous flora is highly interesting though not abundant in species. Two of the commonest trees are the ohia and koa. The former, also called ohia lehua and lehua, resembles, in the appearance of the trunk, our white oak, but bears beautiful clusters of scarlet flowers with long protruding stamens. The koa produces a valuable wood much used in cabinet making, now becoming well known through its use for making ukuleles. Characteristic of the upper forest belt on the high mountains of Hawaii is the mamani, a leguminous tree with long drooping clusters of yellow flowers and long-four-winged pods constricted between the seeds. In the arid regions is found the wiliwili, a deciduous tree with gnarly growth. Its bare branches are conspicuous, as deciduous trees are unusual in the tropics. It has very soft light wood, and bright scarlet seeds. Among the peculiar plants of the islands is the silversword, a strikingly beautiful composite with glistening silvery leaves, which grows only on the slopes of cinder cones in the crater of Haleakala and in a few very limited localities on Hawaii. The family Lobeliaceae is represented by about 100 species belonging to six genera. The numerous arborescent or tree-like species are very peculiar and characteristic. Many of them form slender trunks like small palms, crowned with a large cluster of long narrow leaves. The trunks of some species are as much as 30 or 40 feet high and the large bright-colored flowers are sometimes remarkably beautiful.

The indigenous grasses of the Hawaiian Islands are not numerous. A tall species of Eragrotis is the dominant grass upon the plain between Mauna Loa and Mauna Kea. Upon many of the summits of the high mountain ridges in the regions of heavy rainfall are found open bogs which support a peculiar and interesting flora. Many species form more or less hemispherical tussocks which rise above the general level of the bog. A showy lobelia with numerous large cream-colored flowers as much as three and one-half inches long, peculiar violets, and a sundew are found there. These boggy areas are devoid of trees and sometimes occupy rather extensive areas, the one on Mt. Waialeale covering several square miles.

Three species of tree ferns are found on the Islands, and in some places form extensive forests. These plants produce at the base of the stipe, a great ball of brownish-yellow wool called pulu by the natives and used by them for stuffing pillows and mattresses.

Potato Flake Process

Even with the best care, it is impossible to avoid the decomposition and germination of potatoes, and this generally amounts to 10 per cent, at least in Europe. Within the last twenty years the method of preserving potatoes by a drying process has come into prominence, and especially in Germany where their consumption is considerable. It is stated that the number of plants for this purpose has now greatly increased. Dried potatoes serve mainly as food for stock, but when ground to flour, this material is even introduced into "war bread." Potatoes contain 70 to 85 per cent of water, and this must be reduced to 17 or 18 per cent in the dried product. Starch forms the main part of this dried material, or 16 to 20 per cent of the weight of the potatoes the remainder or 5 per cent being composed of various substances. In spite of many efforts, no success has been obtained in whole drying of potatoes, for a crust is formed during the operation which prevents the proper drying of the inside, and again this does not give a good industrial product. Therefore the potatoes are cut up in pieces or in thin slices and put through the drying machine. A more recent method is to reduce the potatoes to a pulp by a treatment with a jet of steam, then this paste is run between a pair of hot rolls so as to expel the water. The resulting product appears in the shape of very light flakes. The following is a summary description of a plant for treating about one ton of potatoes per hour: After passing the washer and cleaner on the ground floor, the potatoes are brought by a conveyor to the top story and stored in a hopper, and from here they pass by way of an automatic weigher into the machine proper which first reduces them to paste and then runs this between the cylinders. The upper part of the apparatus contains a boiling tank in which the potatoes are treated with exhaust steam from the engine, then they drop into the reducing chamber where sets of paddle wheels or rather cylinders crush up the mass and form the pulp or paste. Without leaving the machine, the paste runs vertically between two large heated cylinders, and the dried and flaky substance coming through is scraped off the cylinders by suitable means. It falls to the bottom of the machine containing a breaker, from whence a pneumatic conveyor takes the small flakes to the storage bins. The paste is not absolutely flattened between the cylinders, for these are mounted some 0.06 inch apart, and before going through, a thin layer of paste has been already formed on the cylinders by suitable scrapers placed at the top and a well-combined feeding device. Great regularity is required in the feed and in the spacing of the cylinders, for if the layer is not of uniform thickness, the thin points become over-dried and take a brownish color. This, however, can be avoided by proper precautions. Steam to the amount of 80 lbs. per 100 lbs. of potatoes is usually employed for heating the rolls, and it is preferable to use live steam from the boilers, and a good economy of steam for the whole process has been secured in this apparatus. The flakes coming from the present process form a very light material which is not easy to put up in sacks, so that it requires to be put under press, using generally the type of press employed for treating hops and the like, or again, a screw press such as is used in sugar-making for pressing beet slices. In this way the flakes are reduced to a smaller mass and are usually put up in 125-lb. sacks. That the system of potato drying has now entered into real industrial practice in Germany, will be seen from the fact that the product from the whole number of plants for the year 1916 amounted to no less than 4,000,000 tons.

Weird Diseases of Africa*

The Story of Strange Parasites which Travel from Man to Man

By Walter B. James, President of the New York Academy of Medicine

FROM earliest times the "Dark Continent" has been known as the home of peculiar men and beasts. Modern science, especially medical science, now teaches us that the minute, even microscopic, life of Africa is no less individual and remarkable, especially its parasitic life, while the symptoms these parasites produce in the unfortunate human beings infested by them are equally weird and generally extraordinarily unpleasant. Many of the diseases produced by these microscopic parasites have been known to physicians—at least as far as their symptoms were concerned—for many years, but only lately have the wider settlement of Africa and the progress of medical science, with the establishment of schools of tropical medicine like those at Liverpool and Harvard University, made the nature of these important diseases clear to us.

In the slavery days in America it was noticed that often in a shipload of slaves that had been captured in the interior of Africa and sent here, a large number would sicken in a peculiar way and then die. They first became dull and listless, then so drowsy that they could be roused only with great difficulty. They refused to eat and then died. Other and subtler symptoms were overlooked. It was natural that illness and death should be ascribed to homesickness. These people, we now realize, had the "sleeping sickness." In their jungle homes they had been bitten by the tsetse flies that had previously bitten persons, or perhaps animals, having this disease, and had conveyed the parasite in this way. For the germ—a worm, not a bacterium—inhabits the blood of its victim, and multiplies there.†

When the blood containing them is taken into the stomach of the fly, the germs multiply there (the fly acting as the intermediate host between the two human beings) and go through a phase in their lives that may be likened—to use a comparison familiar to us all—to one of the stages in the life of a butterfly. The progeny find their way to the salivary glands of the fly and lie there, ready to be injected into the blood of the next person to whom the tsetse turns for a meal. If we take a drop of blood from the finger of a person who is in the advanced stage of sleeping sickness, and put it under a powerful microscope, a remarkable situation is evident. Enormous numbers of the parasites (trypanosomes) are seen rushing about, apparently aimlessly, knocking the blood corpuscles about like so many ninepins, and one wonders that a man could live at all with such weird things going on in his blood

Whole tribes of blacks in Africa have been annihilated by this disease and, as one of the methods of controlling it, it has been suggested that in the parts of Africa most affected, the wild game should be killed off, for it has been shown that animals, too, may be hosts for such parasites and thus help to preserve them. The laws that secure the perpetuation of these minute enemies of man are just as effective as the corresponding laws that seem to us to work for our benefit. Nature is impartial. To be sure she has given us an intelligence that leads us, or ought to lead us, to the use of microscopes, laboratories, and through these to a successful combat with disease; but as an offset to this, she has, as a rule, made us more susceptible to disease than are the lower animals. As far as we know the antelope with trypanosomes in the blood is not made ill by them. The killing of the vast herds of buffalo that once ranged our own western prairies was necessary in order that these lands might support great herds of cattle; and so perhaps when the Dark Continent comes to be settled, it may be necessary to kill most or all of the wild game animals in order to eliminate this terrible disease.

One of the most peculiar of all tropical diseases, and one that is very common in some parts of Africa, is filariasis, caused by a parasite called the filaria. In the blood of persons suffering from it there are found innumerable little worms that can be seen only by the aid of a microscope. These are present only at night in the blood that is circulating. At about five o'clock in the afternoon they begin to appear in the blood, having been hidden away in the body until this time, and then they remain in the circulation until about midnight when they begin to diminish. By eight or nine o'clock in the morning they have all disappeared, and a search of the blood under the microscope after this fails to reveal any. They are now collected in certain large blood vessels deep in the body, especially in the lungs, where they remain hidden until they go out on their next nocturnal excursion.

The parasite is conveyed to human beings by the bite of certain kinds of mosquitoes. The mosquito bites and takes from a man (or from some animal is the case may be) blood which contains these small worms. In the stomach of the mosquito (the intermediate host) the parasite goes through certain definite changes or metamorphoses which are just as necessary to its complete life as are the different phases in the lives of butterflies, moths, and a great many insects. First it escapes from a skin or shell in which it has existed. Then it bores its way through the wall of the mosquito's stomach, and travels forward through the body until it arrives at the base of the bill or proboscis. Here it curls itself up and waits until its host begins to feed upon a warm-blooded animal, when it passes out and finds its way into the circulation of the animal. These filariae behave in a curious way which has the same effect as though dictated by intelligence: that is, when the mosquito, in the absence of animal food, feeds upon fruit, such as bananas, the filaria does not leave its comfortable berth but continues to wait until it has the chance to enter the circulation of a warm-blooded animal. A possible explanation of this, however, is that when a mosquito bites a human being, it first injects a small amount of fluid secreted in its salivary gland. This fluid prevents the blood from coagulating and stopping up the very small bill through which it is drawn. This poisonous substance is the cause of the swelling and itching that follow mosquito bites. It is not known but that the filaria is injected along with this, for it is fair to assume that the insect would not need to inject the secretion when feeding on bananas, as banana juice does not coagulate.

When the parasites find themselves injected into the blood of a man, the "final host," they travel about in the circulation until they find a resting place in one of the larger main lymph vessels, and here they settle down for the rest of their lives, one or more males and one or more females coiled up together. From this point they send out into the blood current every evening innumerable broods of young which are the ones that rest by day and circulate by night; and so the life cycle is run, over and over again. Their nocturnal activity is supposed to be related to the fact that their intermediate hosts, the mosquitoes, carry on their predatory excursions after dark, and therefore it would be of no avail for the little parasites to get into the circulation in the daytime. This is an evidence of Nature's thought for the preservation of a species that, from our point of view, is useless.

When the parent worms have selected a large lymph vessel at the base of a leg or an arm, for instance, and have started in raising a large family, we find that in entire disregard of the comfort of their host, they have soon completely blocked up the vessel, and so produced a slow chronic congestion of the whole limb below. The limb then becomes of enormous size, and from its supposed likeness to the shapeless leg of an elephant, the disease is now called "elephantiasis." When we travel in the tropics, especially in Africa, and see—as one often does a person with enormous enlargement of one arm and hand or one leg and foot, we may be quite sure that it is a case of filariasis, and that there is a colony, as above described, living in the armpit or groin. These cases occur also in other countries, indeed in almost every tropical and subtropical land into which the disease is introduced through travel. It, like malaria, is to be prevented only by getting rid of mosquitoes, the intermediate host.

There is another species of filaria, the *Filaria loa*, very common in Africa, which is transmitted to man by the mangrove fly, a common blood-sucking insect in that country. This worm settles in some of the tissues just beneath the skin, often in the lower eyelids, where it produces uncomfortable swellings.

Another peculiar African parasite is the guinea worm. This, too, has an interesting life history. Little or nothing is known of the male worm, but the female, very slender in diameter although attaining a length of three or four feet in adult life, is found immediately beneath the skin usually of the lower limbs. It has probably been fertilized before entering, and, lying immediately under the skin of its host, when fully grown it pierces this skin and through the minute aperture extrudes countless minute young or larvae, in successive crops. By this time it has caused much irritation and suffering and perhaps disability to the host. The larvae find their way into the water as the natives walk through streams and puddles. In the water they are taken up by a minute aquatic creature called *Cyclops*, which becomes the intermediate host, and within its body they go through one of their life phases.

The *Cyclops* later is taken into the human being's stomach in the drinking water, where it perishes and its minute body is dissolved in the gastric juice. This sets free in the native's stomach the contained living larvae, one or more of which may then succeed in boring through the stomach wall into the body tissues. Now an imperative instinct urges the developing worm to find its way through the body toward the skin in order that it may place its numerous young on the surface, as in this way only can they find the *Cyclops*, necessary to the completion of their life cycle. For this reason, too apparently the female worm seeks the native's lower limbs, for these come most in contact with puddles of water, and it is in puddle water that the *Cyclops* especially abounds. In India, water carriers bearing skins of water on the back are subject to guinea worm of the skin of the back, for in them the parasite's instinct leads it to seek this part of the body.

In all of these organisms and their behavior we see Nature's wonderful methods for the continuation of life of even the lowest species. Design, and successful design, is just as surely seen in the lives of the trypanosomes, the filariae, and the guinea worm as in the wonderful strength of the elephant, the tough hide of the rhinoceros, or the fleetness of the antelope and the ostrich. We realize that we are not Nature's chosen children, but must take our chances with the rest of life; that Nature cares just as much for the parasites that plague us as she does for us who are plagued by them; that she looks to us to take care of ourselves with the weapons she has put into our hands or else perish and make way for others who use her gifts to better purpose. It behooves us, therefore, to use our intellects, our only real weapon in the fight against disease, and to turn for aid to science, which offers us the only hope of victory.

*From the *American Museum Journal*, issued by the American Museum of Natural History, New York.

†This parasite is the *Trypanosoma gambiense*, and is generally called the trypanosome of sleeping sickness. Such a parasite generally has to live in two different kinds of animals in order to round out its complete life, and for each variety of parasite the two hosts are always the same. One is called the "intermediate host," the other, the "final host." In the case of the trypanosome of sleeping sickness, the tsetse fly is the intemediate host, man (or in some cases the antelope) the final host.

Alcohols from Acetylene

THEORETICALLY the preparation of alcohol from acetylene looks easy. By hydrogenation the acetylene CH can be converted into ethylene, CH, which is transformed into ethyl sulphonil acid by means of sulphuric acid; hydrolysis then transforms the sulfonic acid into ethyl alcohol, C_2H_5OH. But neither this direct way, nor various indirect processes, appeared for a time to prove technically profitable, and so the manufacture of alcohol from calcium carbide, which, treated with water, yields acetylene, seemed to remain a problem. Within the last few years, however, several of the large German chemical works have taken patents on the preparation from acetylene, by means of acid mercury salts, of aldehydes, which differ from their alcohols by a deficiency of two atoms of hydrogen. Whether and how far the preparation and hydrogenation of these aldehydes is at present being utilized in Germany we do not know. But the alcohol department of the Swiss government has lately granted a concession for the manufacture of 7,000 tons of alcohol (more later) per year from calcium carbide to the Elektrizitätswerk Lonza, A.G., of Gampel (in the Rhône valley) and Basle, Switzerland. The works are to be opened within 18 months, and are to supply at least 2,500 tons of alcohol to the government. The process of the Lonza Company consists in passing vapors of acetaldehyde, CH_3COH, mixed with an excess of hydrogen, over finely-divided nickel (catalyst); water and alcohol, $CH_3.CH_2OH$, are formed; the former is frozen out, and the excess of hydrogen gas reacts again with aldehyde vapor. The Swiss patent on which the process is based is not yet available over here.—*Engineering*.

Observations on the Friedel-Crafts Reaction

A REVERSAL of the Friedel-Crafts reaction can in general be brought about with more or less facility by the action of aluminum chloride on the alkyl-benzene in presence of a large excess of benzene. In the case of the xylenes alone, this reaction does not succeed, but when polyethyl-, isopropyl-, butyl-, and amyl-benzenes were boiled with aluminum chloride in presence of ten times their weight of benzene, quantities of the monoalkylbenzene, varying in each case were formed according to the scheme:

$$C_6H_4(C_2H_5)_2 + C_6H_6 = 2C_6H_5(C_2H_5)$$

Of the greatest interest, however, was the formation in excellent yield of toluene and cumene from cymene, which is available in large quantities as a by-product of the sulphite-cellulose process. Ninety grms. of cymene in 900 grms. of benzene gave, on boiling for 10 hours with 4.5 grms. of aluminium chloride, a yield of 44 grms. of toluene and 68 grms. of cumene, corresponding to 80 per cent of that theoretically possible, leaving a residue of only 3-4 grms. of gummy material. The polyhalogen derivatives of benzene could not be decomposed by the action of aluminium chloride and benzene in this manner.—*Note in the Journal of the Society of Chemical Industry on a paper by* E. BŒLDTKER and O. M. HALSE in *Bulletin of the Society of Chemists*.

Fig. 1.—Another view of Mount Shasta showing forests about its base, with a sawmill and lumber piles in the foreground. These forests are particularly susceptible to fires from lightning

The Effect of the Weather on Forest Fires*

With Particular Reference to Conditions in California

By Andrew H. Palmer, A.M., (Harvard) Observer, U. S. Weather Bureau, San Francisco, Cal.

THE inauguration of the fire-weather warning service as a part of the work of the U. S. Weather Bureau has opened another interesting field for investigation in meteorology.[1] New problems have presented themselves for solution. The difficulties encountered to date have been largely the result of a lack of data, the absence of normals, and the want of precedent. Though forest fires doubtless occurred long before man appeared on the earth, a systematic record as to their causes extends over comparatively few years. In the United States the matter was not given serious attention until 1880, when a table of forest-fire statistics was prepared as a part of the Tenth Census. The investigation of the relation of weather to forest fires is of even more recent date, while the fire-weather warning service was inaugurated in the Pacific Coast States in 1913 on the recommendation of District Forecaster E. A. Beals.

With reference to their origin, forest fires may be divided into two groups, those caused by man and those caused by nature. While those caused by man are the larger of the two groups, it is not the purpose of this paper to discuss them in detail. Those caused by nature may be subdivided into three groups, (1) those caused by "spontaneous" combustion, (2) those caused by volcanic eruptions, and (3) those caused by lightning.

"Spontaneous" combustion is a direct cause of forest fires only in rare instances, and as an observed source there are few cases on record. However, of the many forest fires of unknown origin it is believed that some, at least, were thus produced. The exudation of oils and other mineral matter from the ground, or the close packing of damp leaves and grass on the forest floor may at times produce chemical reactions which might result in combustion. Forest fires caused by volcanic eruptions had not been recognized in the United States until May 19th, 1915, when an eruption of Lassen Peak in northeastern California was accompanied by a blast of superheated gases which kindled two forest fires in that vicinity.[2] As natural causes of forest fires spontaneous combustion and volcanic eruptions must therefore be considered rare in the United States. The third natural cause, lightning, and its relation to forest fires, is the subject of this discussion.

LIGHTNING AND FOREST FIRES IN THE UNITED STATES

On the national forests of the United States during the five-year period 1911-1915, inclusive, fires were caused as follows: Railroads, 14.4 per cent; campers, 15.6 per cent; brush burning, 7.9 per cent; lumbering, 1.8 per cent; lightning, 29.5 per cent; incendiary, 8.7 per cent; miscellaneous, 5.2 per cent; and unknown, 16.8 per cent. Lightning is a more important factor in causing forest fires than it is in causing fires in cities, the proportion being in the ratio of 7 to 1.

The relation of lightning to forest fires in the United States was studied in 1912 by Mr. Fred G. Plummer, of the United States Forest Service.[3] The more important conclusions reached by Mr. Plummer may be briefly summarized as follows:

Trees are the object most often struck by lightning, because: (*a*) They are the most numerous of all objects, (*b*) as a part of the ground, they extend upward and shorten the distance to a cloud; (*c*) their spreading branches in the air and spreading roots in the ground present the ideal form for conducting an electrical discharge to the earth. Any kind of tree is likely to be struck by lightning. The greatest number struck in any locality will be the dominant species. The likelihood of a tree being struck by lightning is increased: (*a*) If it is taller than surrounding trees; (*b*) if it is isolated; (*c*) if it is on high ground; (*d*) if it is well (deeply) rooted; (*e*) if it is the best conductor at the moment of the flash; that is, if temporary conditions, such as being wet by rain, transform it for the time from a poor conductor to a good one. Lightning may bring about a forest fire by igniting the tree itself or the humus at its base. Many forest fires caused by lightning probably start in the humus. Other things being equal, trees growing in different soils differ slightly in susceptibility to lightning stroke. One study gave these results: Loam, 23 per cent; sand, 18 per cent; clay, 17 per cent; and others 42 per cent. Zones of marked hazard from lightning—due partly to soil variations, partly to mineral deposits, and partly to altitude—are recognized throughout the West. The conductivity of wood is governed by its moisture content and its temperature. Electricity traverses wood more easily in the longitudinal direction of its fibers than across them. About two per cent of trees struck by lightning are ignited. While trees do not differ greatly as to their susceptibility to lighting, they do differ greatly as to inflammability.

LIGHTNING AND FOREST FIRES IN CALIFORNIA

Weather when considered in its relation to the various causes of forest fires may be either a direct or an indirect influence. As an indirect factor it is more impor-

*From the *Monthly Weather Review*, U. S. Dept. of Agriculture.

[1] Concerning the fire-weather warning service see this *Monthly Weather Review*, March, 1916, 44:133-139.

[2] Palmer, A. H. An eruption of Lassen Peak. *Monthly Weather Review*, October, 1916, 44:571-572.

[3] Plummer, Fred G. Lightning in relation to forest fires. Washington, 1912. (Forest Service Bulletin 111.)

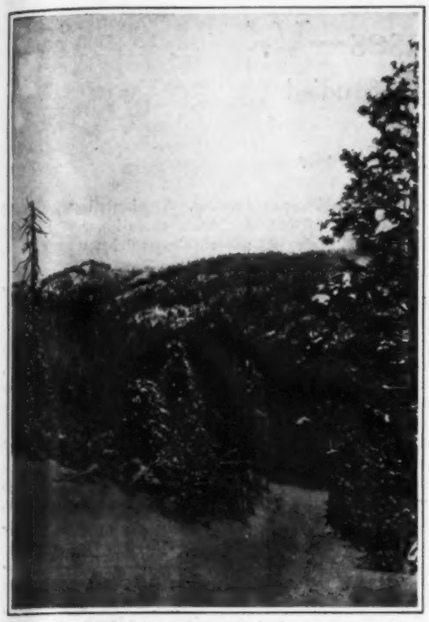


Fig. 2. Midwinter view of a forest near the summit of the Sierra in the region of greatest known snowfall in the United States (Photo by A. H. Caine)

tant than as a direct cause. This arises from a complex combination of conditions, which may be summarized briefly as follows: A drought is a prerequisite of a forest fire. High winds, partly because they accelerate evaporation, but principally because of their fanning effect are second only in importance to droughts as a contributory factor. Hot, northerly, desiccating winds, characteristic of the front portion of an anticyclone, are the most troublesome predisposing cause of forest fires in California. Moreover, the fine dry weather brings out the campers, increases the amount of railroad traffic, as well as the lumbering and brush burning, and thus further contributes to the fire hazard. As an indirect influence, therefore, the importance of the weather in its realation to forest fires is paramount.

It is as a direct cause of forest fires, however, that weather is here considered. Lightning is the sole direct agent through which weather operates to produce fires of this kind. Of the 7,789 fires observed in the national forests of California for the eight-year period, 1908-1915 inclusive, 2,434, or at least 31 per cent, were due to lightning. Of the 6,353 fires in which the causes were determined, at least 38 per cent were due to lightning, but it is recognized that yet larger numbers might be nearer the truth since records are known to be incomplete for the present purpose. It was the most important single cause of forest fires. Both for the United States as a whole, with 29.5 per cent, and for California with 31 per cent lightning leads, while railroads were fourth and fifth in importance, respectively. The difference is due partly to the relatively greater number of thunderstorms in the California forests, and partly to the relatively smaller amount of railroad traffic.

Lightning rarely occurs independently of a thunderstorm. Thunderstorms, however, are of various kinds. Those along the coast are limited almost entirely to the winter half-year, and are invariably of cyclonic origin, caused by the overrunning of a relatively warm stratum of air by one relatively colder. They come in ready-made from the ocean, are of short duration, and of feeble intensity, with few electric discharges in the form of lightning. But one thunderstorm occurs each year on the average at San Francisco. In the mountain regions, however, where most of the forests are situated, thunderstorms occur throughout the year, though they are most frequent during the summer months. They are often violent and long continued, and are accompanied by numerous lightning discharges. Under the strong insolation which California receives during the long summer days, local heating of the air results in vertical convection which produces cumulus clouds, a characteristic feature of the landscape during afternoon hours. Under favorable conditions, which include moderate or high humidity and calms or light winds, these cumulus clouds become overdeveloped and produce cumulo-nimbus, or thunderheads. The photograph shown on the cover page was taken by Mr. C. A. Gilchrist and is here reproduced through his courtesy. It shows a stage in the formation of a thunderstorm over Mount Shasta. This particular storm subsequently attained great violence, and was accompanied by destructive lightning. As indicated in the foreground, the view was taken during the haying time of early summer, when snow was still abundant on Shasta. The summit of this mountain is 14,380 feet above sea level and more than two miles above the floor of the valley shown in the foreground. It would thus appear that the cirrus cap at the top of the ascending column must have been fully five miles above the top of the mountain, or about eight miles above sea level.

During the summer months the weather of California is dominated by the great North Pacific HIGH. For this reason the surface winds are local in origin, while the upper air is almost stagnant, as far as horizontal currents are concerned. The amount of rain which accompanies these local thunderstorms varies greatly, though it is generally light. No relation is apparent between the amount of rain which falls during one of these storms and the number of lightning flashes. However, it has been observed that the deeper the storm, that is, the greater the height reached by the ascending current, the more frequent are the electrical discharges. When lightning strikes a forest tree the accompanying rainfall will often prevent ignition, thus accounting in part, at least, for the fact that 98 per cent of trees struck by lightning are not set on fire. Moreover, a rainfall of 0.25 inch will make it practically impossible for a fire to spread. Many thunderstorms, however, are accompanied by no measurable rainfall. Furthermore, when anticyclonic conditions are well developed the resulting surface winds are from the north or northeast, the direction of most dangerous "fire winds" in the State. Partly because of their excessive dryness, but principally because of their fanning effect, these winds are most dreaded by foresters while fighting a forest fire.

FIRES FROM LIGHTNING VARY WITH DIFFERENT FORESTS

It might be inferred from the foregoing that lightning as a factor in causing forest fires, varies with different forests as well as with different years. That such is the case may be noted from Table 1, which shows the number of fires on the 18 national forests during the four years, 1912-1915, inclusive, and the number and percentage caused by lightning. It is noteworthy that the Santa Barbara Forest, the one nearest the coast, had the least number of lightning fires, and not a single fire caused by lightning in 1912 or in 1915, though 50 fires occurred during the former year and 147 fires during the latter year. On the other hand the Modoc Forest, the one farthest distant from the coast and well up in the mountains, had relatively the greatest number of lightning fires, 84.6 per cent of those in 1912 and 72.2 per cent of those in 1915 having been so caused. The great contrast is due principally to the difference in the frequency and in the nature of thunderstorms in the two forests, the one at a low altitude along the coast, the other at a great altitude in the interior.

TABLE 1.—*Fires in the national forests in California showing number and percentage caused by lightning during 1912-1915.*

[Statistics by U. S. Forest Service.]

Forest.	1912			1913			1914			1915		
	Total number of fires.	Lightning fires.		Total number of fires.	Lightning fires.		Total number of fires.	Lightning fires.		Total number of fires.	Lightning fires.	
	No.	No.	P.ct.	No.	No.	P.ct.	No.	No.	P.ct.	No.	No.	P.ct.
Angeles	72	2	3	130	37	31	138	4	3	116	7	6
California	20	5	25	76	35	46	48	6	12	24	1	4
Cleveland	41	0	0	72	43	60	78	14	18	37	1	3
Eldorado	52	2	9	74	10	14	46	15	33	74	5	7
Inyo	2	1	33	5	3	60	4	3	75	3	0	0
Klamath	85	32	38	126	85	67	163	103	64	219	37	17
Lassen	96	40	62	77	48	62	95	60	63	92	34	37
Modoc	26	22	85	49	36	73	54	35	60	72	52	72
Mono	4	2	50	6	6	100	5	1	20	15	5	33
Monterey	5	0	0	8	0	0	2	0	0	15	2	13
Plumas	96	30	31	136	84	54	149	32	22	108	27	25
Santa Barbara	50	0	0	63	5	8	57	3	5	147	0	0
Sequoia	80	3	4	127	91	72	81	47	58	36	10	28
Shasta	58	29	52	129	62	48	133	34	26	196	77	39
Sierra	67	1	1	172	77	45	88	30	34	27	4	11
Stanislaus	32	3	13	101	49	48	58	12	21	30	4	13
Tahoe	60	5	8	130	45	35	223	53	24	163	17	10
Trinity	37	29	78	137	98	72	83	26	53	193	37	19
Total	812	206	25	1,625	804	49	1,468	480	33	1,527	310	20

Total number of fires during 4-year period..... 5,435
Number of lightning fires during same period..... 1,800
Average annual number of lightning fires..... 450
Percentage of lightning fires to total number..... 33

Thunderstorms, like certain other elements of the weather, vary in number from year to year, and for this reason the number of lightning fires varies. As may be inferred from Figures 2 and 3, which are winter photographs of California forests, thunderstorms which occur during that season rarely start forest fires. The humus and forest litter is then either snow covered or has been saturated by rains. In these mountain regions 85 per cent of all the precipitation received during the course of the year falls in the form of snow. Moreover, the violent thunderstorms originating from local surface heating are then infrequent. The great majority of forest fires in California occur during the months July to September, inclusive. The winter snow is important in another respect. It is recognized that susceptibility to forest fires during the summer months depends not only upon the character of that season, but also upon the amount of snow which fell the preceding winter and the manner and rate of its disappearance upon melting. The importance of these considerations is apparent when the dryness of the forest floor is recognized as a factor. The average man, unacquainted with forest conditions, would marvel at the ease with which such an apparently green forest would ignite, even though the snow has but recently melted. Figure 1 is another photograph of Mount Shasta, showing a forest and lumber piles at its base. Sparks blowing from the smokestacks of sawmills like the one here shown were formerly a prolific source of forest and lumber-yard fires. The adoption of efficient spark screens has now largely eliminated that cause of fires.

Fig. 3. The absence of wind, as indicated by the snow on the trees, and the deep snow cover account for the infrequent forest fires during the winter. (Photo by A. H. Caine)

It should be borne in mind that all forest fires started as small fires. When lightning strikes a tree it may ignite the tree or the débris and undergrowth beneath, the fire later spreading if conditions are favorable. Of the three kinds of fires recognized by the Forest Service, all may be caused by lightning. These three kinds are (1) ground fires, which smolder indefinitely in the ground, consuming humus, duff, and roots of trees; (2) surface fires, which spread over the surface of the forest floor, fed by undergrowth and débris; and (3) crown fires, which consume the entire forest cover.

According to Mr. Plummer[8] scars traceable in the annual rings of the famous Big Trees of California suggest that great forest fires occurred about the years 245, 1441, 1580, and 1797 A. D. It is known that the American Indians have occasionally set fire to forests in order to clear the land for agriculture, to drive game, or to impede the progress of an enemy, but it is more likely that these great fires were kindled by lightning. These trees also refute the popular supersitition that lightning never strikes twice in the same place. Certain trees are known to have been struck eight times, with no other apparent effect than a dwarfed growth.

CONCLUSION

The importance of lightning as a cause of forest fires may be judged from the foregoing statements. Being of natural origin, lightning is one of the factors which can never be eliminated. However, the situation is not hopeless. The main hope lies in the anticipation of fires and the making available of facilities to subdue them when they occur. The fire-weather warning service gives hope of reward. Thunderstorms with their destructive lightning form simply one of the elements which must be considered. In this, as in other branches, the dominant need is for more field work in order to secure more complete data with reference to each individual forest. As this information is secured further advance may be expected of meteorology in general, and of fire-weather forecasting in particular.

[8] Plummer, Fred G. Forest fires. Washington, 1912. (Forest Service Bulletin 117.)

The Electrical Properties of Gases—V.*

Which Enable Important Problems in Physics To Be Studied

By Sir J. J. Thomson, O.M., P.R.S.

CONCLUDED FROM SCIENTIFIC AMERICAN SUPPLEMENT No. 2170, PAGE 70, AUGUST 4, 1917

In this last lecture of his course on the above subject Sir Joseph proposed, he said, to consider the application of some of the results demonstrated on previous occasions to certain important questions in general physics. Of these, the first was, What was responsible for the luminosity of gases? He had shown that in a discharge tube the luminosity varied from point to point, so that in some parts there was an absence of light, while other parts were bright. These variations in the luminous appearances ought to give some information as to what influenced the emission of light from gases. The theory of striations showed that in certain parts of the tubes in which they appeared there was no ionization of the gas present. The operation of ionization consisted in splitting up the molecules of a gas into negative and positive residues, and it might be that the production of light was the result of ionization. If so, two questions arose. In the first place, was ionization a necessary condition for luminosity? or, in other words, when we got light, did we also always have ionization? There was ample evidence that ionization had certainly a good deal to do with luminosity, as he had shown in an experiment (previously reported) in which electrons produced from a hot wire were squirted through a gas. In this case, unless the speed of the electrons was increased beyond a certain definite limit no visible radiation was emitted by the gas. When, however, the speed of the electron exceeded a certain critical value, the gas became luminous and concurrently the strength of the current went up, which was evidence of ionization of the gas. There was thus undoubtedly a close correspondence between ionization and luminosity. Admitting this close association between the two, the question remained as to whether ionization was essential.

The evidence showed that there were cases in which luminosity was not attended by ionization. In this connection it was important to bear in mind that it was very much easier to detect ionization than luminosity. The tests for ionization were of extraordinary delicacy, as was indicated by the fact that it was possible to detect four or five ions in a cubic centimeter of gas containing some 2.75×10^{19} molecules. If this ionization were accompanied by radiation the energy of this was far too small to be detected by the eye or by the longest possible exposure of a photographic plate. The tests for ionization were, therefore, so delicate that it was possible to say with confidence that some few cases were known in which we had luminosity without ionization. One of the most interesting of these was afforded by iodine vapor, which phosphoresced when exposed to the action of ultra-violet light. Tests for ionization when the gas was thus emitting light showed no conductivity. Nevertheless it had been found that, if not actually ionized, some progress had been made toward such a condition, as the vapor was more easily ionized when phosphorescing than when in its natural condition.

Anthracene vapors, and those of some other compounds characterized by the presence of the benzene ring, fluoresced under the action of ultra-violet light, and here, again, there was no ionization. These cases seemed to be exceptional, but were perhaps only apparently so, the luminosity being due to a process very similar in kind to ionization, which consisted essentially in the separation of an electron from an atom of the gas or vapor. It was, however, possible to imagine cases in which the shift of the electron was not sufficient to take it outside of the sphere of action of the atom.

The electrons might be looked on as the mortar binding together two atoms of a molecule. The suggestion therefore arose that the luminosity unaccompanied by ionization was due not to the ejection of an electron, but to its transference from one position in the molecule to another. In chemical language this would correspond to an alteration in the bonds connecting the atoms together. Thus two atoms doubly linked in the normal state, as O=O, might be altered so as to be singly linked thus, —O—O—. We would thus get a movement of an electron of the same kind as in complete ionization. But this movement would be wholly intra-atomic or intra-molecular. He thought that some action of this kind was responsible for phosphorescence.

In certain cold flames, again, there were no traces of ionization, but there might be a sort of quasi-ionization, giving similar results in the matter of light emission.

Again, there were many cases of ionization unaccompanied by any visible production of light. An instance of this was afforded when gases were ionized by Röntgen rays. This might be due to the difficulty of detecting light. Summing up, it might be said that there was certainly a close connection between ionization and luminosity, but it would be going too far to say that complete ionisation was essential to the production of light.

In any case ionization was never permanent. The missing electron was, sooner or later, always replaced, and a point of interest arose as to at what state of the process between the loss and the replacement of an electron was luminosity produced. Was it when the electron returned or when it was ejected? The study of striations threw no light on this matter, but there was independent evidence which indicated that it was the return of an electron to the atom which gave rise to luminosity.

He had, the lecturer continued, made some experiments not very long ago which threw light on this point. He was investigating the production of ionization by a form of radiation intermediate between ordinary Röntgen rays and ordinary light. These "soft Röntgen rays" were produced by the impingement on solid particles of electrons moving at different speeds. It turned out that no radiation whatever was produced until the energy given to these electrons exceeded that due to a fall through some 15 volts to 20 volts. At this limit a small amount of these soft radiations was emitted, which increased as the voltage was raised. The type, however, varied very little even when the fall was changed from 20 volts up to 100 volts or 200 volts. Through all this range by far the greater part of the radiation produced was of the same type as that produced by the 20-volt drop. It was not wholly of this type, since there was an admixture with other constituent radiations which increased with the energy given to the electrons, but nevertheless, the major proportion of the total remained unchanged in character in spite of the large increase in voltage. A point was, however, at length reached in which a further addition to the energy of the electrons led to the emission of a different type of radiation, which in its turn persisted through a considerable range of voltage. As before, there were stray constituents depending upon the energy supply.

These peculiarities were easily explainable if the radiation was due to the replacement of a lost electron. The energy involved in this would be always the same, whatever the speed of the electrons which had effected the original ionization. With a certain energy the colliding particles electrons would be detached from one position in the atom, but to knock off other electrons from other positions the energy would have to be increased. There was thus reason for believeing that in the main luminosity resulted from the return of an electron to the positive residue of an atom.

It was possible to have several types of positive residues. Positive rays, for example, were found consisting of atoms carrying one charge, and also of atoms carrying two positive charges. Thus, if an electron returned to an atom which had only one positive charge the result might well be different than when it returned to an atom showing two positive charges. Indeed, Stark had been able to distinguish certain spectral lines as due to residues having one positive charge and others as due to doubly-charged residues. This fact was connected with some extraordinary changes in the color of certain gases when the character of the discharge through them was altered. A notable instance was afforded by argon. A tube charged with this gas emitted light of a purplish color when the discharge through it was derived from an ordinary induction coil; but when Leyden jars were included in the circuit the light was of a bright, steel blue color. Hence by altering the conditions of the discharge it was possible to change the type of luminosity given out. The difference might be due to there being a great preponderance of singly-charged residues in the one case and of doubly-charged residues in the other.

Although the luminosity was due to the return of an electron to the atom, previous ionization was essential. More energy was required to ionize one kind of atom than another. Hence in a mixture one kind might be excited to luminous emission and not the other.

This the lecturer illustrated by placing a bulb, filled with a mixture of hydrogen and chlorine, as secondary to a coil traversed by very rapidly alternating currents. The hydrogen was, he stated, more easily ionized than the chlorine, and the green light from the latter gas was accordingly only visible near the surface of the bulb, where the field was very strong. The inner layers showed only the red of hydrogen, as in these inner regions the negative particles did not acquire sufficient energy from the field to ionize the chlorine.

Summing up to produce luminosity it was necessary in general to have positively electrified atoms to which electrons could return. There was one method known, the lecturer continued, by which it was possible to obtain such positively electrified particles in great number. It would be a complicated matter to use discharge tubes filled with potassium or sodium vapors; but a large supply of positively charged atoms of these elements could be obtained by coating the positive electrode of a tube with a paste made up out of their iodides or other similar salts. When the discharge passed the positive electrode got hot and the salts evaporated, and a stream of positively electrified ions was obtained, which had very considerable luminosity.

A point of some difficulty arose in attributing luminosity to the return of an electron. There was reason to believe that the hydrogen atom had only one electron, since in positive ray analysis no hydrogen atom was ever found to have lost more than one electron. How was it, then, that from this single positive residue and this single electron that we got the complicated hydrogen spectrum. He did not refer here to the second spectrum of hydrogen, with its almost infinite complication, but to the ordinary spectrum as represented by the Balmer series of lines. It would be natural to expect that from a combination of one electron with one positive particle symmetrically placed, but one spectral line should result, but actually there was a practically infinite number.

One view advanced amounted to this: that there were practically an infinite number of kinds of hydrogen atoms. It was, in fact, quite possible to make up an infinite number of neutral atoms consisting of a positive particle in equilibrium with a negative one. Such an equilibrium showed that there must be a repulsive force to keep the two apart. One way of providing for this was to attribute the repulsion to centrifugal force, and by suitably altering the speed it was possible to get, on this hypothesis, a hydrogen atom of any size. This, however, did more than was required, since in that case the series of lines would merge into a continuous spectrum. Something more was necessary. Those acquainted with hydro-mechanics would know that in a vortex ring in a liquid the speed of a particle was governed by the relation $v\,r = \text{constant}$, where v denoted the speed and r the radius of the path. This held wherever might be the center enclosed. If, however, two centers of vorticity were enclosed the value of $v\,r$ would be doubled, and if three centers were enclosed it would be tripled. Assuming the repulsion of the electron in a hydrogen atom to be due to centrifugal force, the value of this force was $\frac{v^2}{r}$, which might be written as $\frac{v^2 r^2}{r^3}$. For equilibrium in its orbit this force must balance the attraction between the electron and the positive charge. Hence, if $v\,r$ represented circulation round a vortex ring the electron would be in equilibrium near in if it enclosed one center, but if it enclosed three it must be farther out. Hence possible positions varied by jumps.

This represented one way of regarding the origin of the hydrogen spectrum, but it was not in accord with chemistry, since the amount of energy required to ionize the atom would differ with the different positions of the electron. The chemical properties depended upon the energy required to separate the electron from its atom, and hence on the view above explained we ought to find that some hydrogen atoms were more electro-negative than others. So far there was no evidence in support of this. It had, however, to be borne in mind that much yet remained to be discovered, and that such a difference would not be found unless specially looked for. It was, therefore, quite possible that if chemists set themselves to investigate this point confirmatory evidence might be forthcoming. At the present time, however, the theory just explained was not backed up by any chemical evidence.

There was, however, another way of regarding the matter. The field of force between the electron and the positive charge might be represented by lines of force stretching from one to the other.

If we regarded these as having a material existence and shot an electron between them at a high speed they would be disturbed, and we might thus get vibrations of the lines of force independent of the electron to which

*From a report in *Engineering*.

they were attached. In this way we might derive a very complicated spectrum from a system consisting of but one positive and one negative particle.

Coming next to the structure of the atom, the lecturer said he would consider first the characteristic Röntgen radiations which would be derived from atoms. These Röntgen rays were an extreme case of light rays, and were just as characteristic of a particular atom as was its ordinary spectrum. These Röntgen-ray spectra were, however, of such simplicity that the laws which governed them sprang out in a way which was not possible with such entangled sets of lines as formed the luminous spectra.

Mr. Mosley (whose death on active service had been so great a loss to science) had shown that the frequency of the characteristic Röntgen radiation from any element was proportional to the square of the "atomic number." This atomic number must be distinguished from the atomic weight. It denoted merely the order in which the elements came when arranged according to their atomic weights. Thus the atomic number of hydrogen was 1, that of helium 2, of lithium 3, and so on. Theory indicated that the atomic number was proportional to the number of free electrons in the atom. In formulating his law Mosley had to suppose that two elements still remained to be discovered.

The connection between the atomic number and the atomic weight was of extreme interest. The most characteristic feature of the list of atomic weights was their tendency to approximate to whole numbers, although the differences were appreciable in some cases. In the case of the smaller atomic weights the difference was nevertheless astonishingly small, and it was quite out of the question that it could be a mere matter of chance. We were for this and other reasons led to believe that the atom was built up of a number of positively charged units, each having a weight equal to that of the hydrogen atom, combined together. The positive charge in the atom was proportional to the number of these units and hence to the atomic weight. We knew, however, that the number of free electrons was not equal to the atomic weight but to the atomic number.

How did the positive units stick together? It was not legitimate to suppose that the law of the forces between charges so closely adjacent was the same as when they when separated by considerable distances. Still, whatever the law governing it, the repulsion existed, and some sort of cement was required to keep the charges together, and this was provided by a certain number of electrons mixed up with the positively charged units. These had to be brought down to the center of the atom from the exterior, thus reducing the number of free electrons below that of the positive charges.

A mathematical investigation of the conditions of the stability of such a nucleus was for the present almost impossible, but if we studied the list of the atomic weights certain rules were apparent. Confining attention to elements with atomic weights under 40, which were those most accurately known, it was to be noted that if the atomic weight were divided by 4, the residue was always either zero or 3. There were but two exceptions to this rule, viz.: beryllium, with an atomic weight of 9, and nitrogen, of which the atomic weight was 14. With numbers given at random there ought to be as many with one remainder as with any other, but the data showed that the elements could be divided into two types, with which the remainders, after dividing the atomic weights by 4, were zero and 3 respectively. This law would result if we supposed that the number of electrons going to produce stability of the nucleus must be an even number and at least one-half the number of positive charges they bound together. If we tried to build up the elements on this rule with one positive charge no electron cement was necessary, and we thus had hydrogen with one positive unit. With two positive units we should require by the proposed rule at least one-half the number of cementing electrons, but the half of two was an odd number, and we had, accordingly, no element of atomic weight 2. In the case of three nuclei we must have at least one-half the number of nuclear electrons; that was to say, in this case, two, which was an even number, and an element of atomic weight 3 was, therefore, possible. It would be remembered that in the course of his positive-ray analyses he had very frequently found particles with an apparent atomic weight of 3. It would, however, be very difficult to get chemical evidence of the existence of this body, since two of the positive charges were neutralized by the central electrons, and the electrical field produced would be indistinguishable from that of hydrogen. It was this field which determined the chemical properties of the atom, and hence an element of atomic weight 3 would be very elusive to chemical tests. Its constitution would be identical with that of hydrogen and, in fact, he had in the past considered his find as H_3. It would seem, however, that an element of this atomic weight might possibly exist, differing from hydrogen only in other than chemical properties. Coming to a nucleus of four, this, by the rule, would be stable with two nuclear electrons, as half of four was an even number. We had accordingly helium, which was known to have two free electrons. With five positive charges at the center of the atom, the number of cementing electrons could not be three, as this was an odd number. Four was a possible number, but in that case the chemical properties would be identical with those of hydrogen, and uch an element was not known to exist. With six positive charges we could not have three cementing electrons, since the three was odd, and with four the element having but two free electrons would not be distinguishable chemically from helium. With seven, four cementing electrons would serve, and we got lithium. With eight, there was a gap, no element of this atomic weight being known, while beryllium, with atomic weight 9, should not exist. Nitrogen provided the only other anomaly. With ten we could not have five bonding electrons, as five was an odd number, and an element of atomic weight 10 was unknown. With eleven, we got six bound electrons and five free ones, corresponding to boron, and twelve positive units gave us carbon. Proceeding in this way it was possible to account for all the elements up to atomic weight 40, with the two exceptions already stated.

Above this limit the same principle could be applied, but more negative charges than one-half the total of positive ones were needed as cement. In fact, a discontinuity appeared with calcium, after which the positive charges required four more than half their number of electrons mixed with them to hold them together. Later on further critical stages became evident at which the binding electrons had to be in still greater excess over the figure of one-half the positive units. The elements located where one flight ended and the next began had properties of an interesting character. There were long series throughout which the law governing the number of bonding electrons was constant, and then came points of discontinuity at which a fresh series commenced.

In work of this kind it was important to take the atomic weight as proportional to that of hydrogen. Chemists preferred to refer the atomic weights to oxygen, as being more convenient for their purposes. Taking hydrogen as the basis, it would be found that with very few exceptions the atomic weight was always less than the nearest whole number. To this rule silicon and chlorine provided the only exceptions. It was known that mass was not constant, but depended on the amount of energy represented. If we had systems built up with positive and negative particles which required a large amount of energy to pull apart, the mass of the system would not be the same as that of its constituents when separated from each other, but would be slightly more than in the latter case. In actual atoms the mass was slightly in defect. The amount of the change in mass could be calculated by the rule that if a mass equal to the missing quantity were set to move with the velocity of light the kinetic energy represented would be equal to the work required to bring the constituents of the atom together again. With the exception of silicon and chlorine, the mass of the atom was always just a little less than the sum of its constituents. This fact indicated that work must have been done in pushing the constituents together. The amount of this could be estimated by the difference between the atomic weight and the nearest whole number. The amount thus determined represented a greater amount of energy than had yet been found in the α particles emitted by radio-active bodies. There was thus a great store of energy in the interior of ordinary atoms.

The biggest discrepancy in the rule that atomic weights approximated to whole numbers was found in the case of chlorine, the atomic weight of which, referred to that of hydrogen, was 35.2. [Referred to oxygen as 16 it is 35.46.] If this figure could be relied on it would mean that no work had been expended in building up the chlorine atom. He was inclined to think, however, that the real reason for the difference between chlorine and other elements was that chlorine as we knew it was mixed up with molecules of another body with identical chemical properties, but of slightly greater atomic weight. If the true figure for chlorine was 35 there would be 18 inside electrons and 17 free electrons. An atom with atomic weight 39 and with 22 nuclear electrons would also have 17 free electrons and the same chemical properties as chlorine. The addition of 3 per cent of such a body to true chlorine would account for the whole of the discrepancy. He hoped to attack this question by the positive-ray method of analysis, and to find out whether any evidence could be obtained of a chlorine with an atomic weight 4 units greater than that of ordinary chlorine.

Aircraft Testing

A GENERAL account of some of the methods of testing the performance of aeroplanes as practised by the Testing Squadron of the Royal Flying Corps was given by Captain H. T. Tizard before the Aeronautical Society.

He stated that, so far as England is concerned, the general principles of the scientific testing of aeroplanes were first laid down at the Royal Aircraft Factory. The methods of reduction adopted by the Flying Corps were based to a considerable extent on those of the R.A.F., with modifications jointly agreed upon; they have been still further modified since, and recently a joint discussion of the points at issue has led to the naval and military tests being coördinated, so that all official tests are now reduced to the same standard.

CLIMBING TEST

In order to reduce the results of a climbing test to standard atmosphere (*i. e.*, the density of dry air at 760 mm. pressure and a temperature of 16 deg. C.) observations are required at intervals of the time from the start and of the height reached at those intervals. Here the difficulty arises that there is no instrument which records height with accuracy. The aneroid has faults partly unavoidable and partly due to those who first laid down the conditions of its manufacture. As now graduated it will give the correct height only if the atmosphere has a uniform temperature of 50 deg. F. (10 deg. C.) from the ground upward. This temperature is much too high, and only on the hottest days in summer, and even then very rarely, will the average temperature between the ground and 20,800 feet be as high as 50 deg. F. In winter or cold days the reading may be 2,000 ft. too high at 16,000 feet, the aneroid showing 16,000 feet when the real height is only 14,000 feet.

Hence a temperature correction must be applied, and on account of its magnitude it is important that observations of temperature should be made during every test. For this purpose a special thermometer is attached to a strut of the machine, well away from the fuselage and clear of any warm air coming from the engine. If the temperature is noted at the same time as the aneroid reading, both the atmospheric pressure and temperature at the point are known, and the density can be calculated or read off from curves drawn for the purpose.

The observations necessary (after noting the gross aeroplane weight and the net or useful weight carried) are therefore (1) aneroid height every 1,000 feet; (2) time from the start of the climb; and (3) temperature, with (4) air speed and (5) engine revolutions at frequent intervals. The observed times are then plotted on squared paper against the aneroid heights; and from the curve drawn through them the rate of climb at any part can be obtained (also in aneroid feet) by measuring the tangent to the curve at the point. This is done for every 1,000 feet by aneroid, and the true rate of climb is then obtained by multiplying the aneroid rate by the correction factor corresponding to the observed temperature. The time rates are then plotted afresh against standard heights, and the curve gives the rates of climb corresponding to the standard heights of 1,000 feet, 2,000 feet, 3,000 feet, etc.

At least two climbing tests, and if time permits three or more, are made of every new machine up to 16,000 feet, or more by aneroid. The final results given are the average of the tests, and represent as closely as possible the performance on a standard day, with temperature effects, up and down currents, and other errors eliminated.

SPEED TESTS

After reaching 16,000 feet, or whatever it is, the flyer's next duty is to measure the speed flying level by air-speed indicator at regular intervals of height (generally 2,000 feet) from the highest point downwards. The air speed indicator reads too low at great heights, and its readings must be corrected by dividing by the square root of the density of the air. Even the corrected speeds will be true only if there are no disturbances due to the pressure head being in close proximity to struts or wings.

Hence it is always necessary to calibrate the air speed meter, and the only way of doing this is to measure the real air speed by actual timed observations from the ground. One method is to use two camera-obscuras, one of which points vertically upward and the other is set up sloping toward the vertical one. At one important testing center the cameras are a mile apart But when a machine has to be tested quickly and low clouds or other causes prevent the camera test from being carried out, it becomes necessary to rely on measurement of speeds near the ground, the aeroplane being flown about 10 feet off the ground and timed over a measured run by two observers, one at each end of the course.

An Apparatus for Determining Freezing Point Lowering*

By R. G. Van Name and W. G. Brown

THE apparatus here described and figured operates, like the familiar Beckmann method, on the undercooling principle. It differs from others of this type both in the manner of effecting the undercooling, and in the fact that the whole process, undercooling, inoculating, freezing, and remelting in preparation for a new determination, is carried out in a vacuum walled container (Dewar tube).

With any type of freezing point apparatus the need of effective heat insulation of the mixture of solid solvent and solution during the period of temperature observation is self-evident, and the use of a Dewar tube as container is a recognized feature of at least one well-known method, that of Richards. The convenience and accuracy of the Richards method are unquestioned, but it has the disadvantage shared by others of its type, that the concentration of the solution whose freezing point is measured cannot be definitely fixed beforehand. For this and other reasons methods of the undercooling type are in certain cases to be preferred, both for very exact measurements and for those of lower accuracy.

The form of apparatus which we have used is represented in Fig. 1, which shows all the important parts, but omits the mechanism for operating the stirrer, and a few other minor details. The Dewar tube, of about one-half liter capacity, is surrounded by a projecting jacket of tin, AA and is closed by a cork stopper, B, perforated in four places to admit (1) the Beckmann thermometer, (2) the stem of the glass stirrer C, (3) a short tube, not shown in the figure, ordinarily kept stoppered, through which inoculation is effected, and (4) the cooling device D. This cooling device, consisting of two concentric glass tubes through which cold brine[1] is circulated, is the essential feature of the apparatus.

As the brine enters by the inner tube its temperature is shown by a small thermometer. At the beginning of a determination the inner tube is drawn up slightly so that there is a free passage between the two tubes at the bottom. The brine passes down the inner tube, rises between the inner and outer tubes, thus exerting its cooling effect on the liquid outside, and finally escapes through the side tube. When the desired degree of undercooling has been obtained, the inner tube is lowered until in close contact with the outer tube at the bottom, where it has been ground to a fit, thus stopping the circulation through the lower part of the cooling system. This is the position shown in Fig. 1. The lowering of the inner tube uncovers a hole in that tube, previously covered by the rubber stopper E, and the brine now circulates only through the upper portion of the cooling tube which projects from the apparatus. So long as the inner tube is in this lower position the upper part of the cooling device is thus kept at a temperature below that of the solution, and by an amount which is easily regulated. This is an important point since it insures that such transfer of heat as occurs through the cooling tube shall be outward rather than inward, so that instead of promoting leakage of heat into the apparatus, as it might otherwise do, the cooling device actually tends to compensate for such leakage as occurs elsewhere.

Fig. 2 shows the details of the cooling device, though without its thermometer. The inner tube is here shown raised, in the position for undercooling. All the upper part of the cooling tube, down to a point below the level of the liquid into which it dips, is surrounded by a narrow vacuum jacket, so that the cooling effect is exerted only below the surface of the liquid. This prevents accumulation of frost on the cooling tube above the liquid, which would be apt to cause premature freezing, and therefore permits the use of a much colder brine, making the whole determination a great deal more rapid. An unjacketed cooling tube can be used with equally accurate results, but only at a considerable sacrifice in convenience.

The brine is drawn through a strainer of fine wire gauze from a large ice and salt mixture placed slightly above the level of the apparatus, and circulates through the system by gravity, the rate of flow being regulated by screw pinch-cocks on the rubber connecting tubes. It is collected in another receiver and from time to time returned by hand to the freezing mixture.

*From the *American Journal of Science*.

[1]It is assumed here and in what follows that we are dealing with the measurement of freezing point of water solutions, since this is by far the commonest and most important case, and is the one to which our practical experience with the apparatus has been confined. The adaptability of the method for other solvents, of widely differing melting points, is, however, quite obvious.

A side branch in the tube which conveys the brine to the apparatus is connected with the water main so that when desired the brine temperature may be raised by admitting water in regulated amounts. To insure thorough mixing of the two liquids they pass, just beyond their point of meeting, through a roll of fine wire gauze, so arranged as to be easily accessible for cleaning (Fig. 2, G). After each determination, the ice which has separated is melted by shutting off the brine altogether and permitting water alone to flow through the cooling tube for a short time.

FIG. 1.

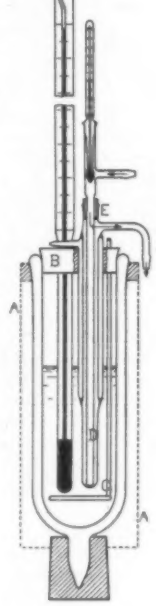
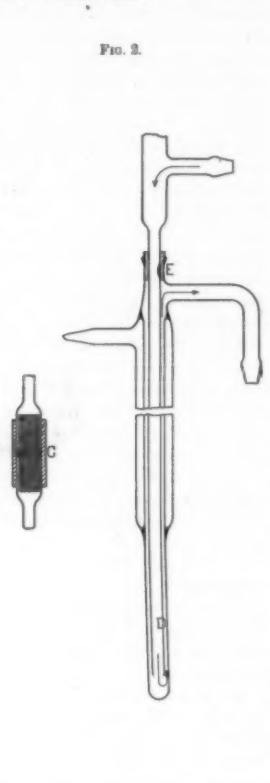


FIG. 2.



The stirrer is driven by a motor, and except for a brief interval at the time of inoculation, is kept in motion throughout the experiment. The successive steps in determining the freezing point of a solution are as follows: The solution (of known concentration) is placed in the apparatus, the stirrer set in motion and the flow of brine started. When the desired degree of undercooling has been reached the inner tube of the cooling device is lowered to the position shown in Fig. 1. After waiting a short time for temperature equilibrium the thermometer is read (this reading is used only in calculating the degree of undercooling) and the inoculation effected in the usual way, whereupon the temperature rises and becomes constant at the freezing point. Owing to the good heat insulation the constancy of the freezing point is almost as perfect when working with a solution as it is with pure water. This eliminates the necessity for considering the so-called "convergence temperature," and makes the calculation of the correction for undercooling simple and free from uncertainty.[2]

The effective heat insulation is due not only to the use of the Dewar vessel but also to the fact, mentioned above, that the flow of brine through the upper part of the cooling tube tends to compensate for the heat leakage and the heat generated by stirring.

With regard to the possibility of using this form of apparatus for very accurate measurements we would call attention to the fact that by suitable provision for a closer regulation of the temperature, rate of flow, and path of the brine, this compensation could be made approximately complete, thus permitting the temperature readings to be made under practically ideal conditions. The accuracy would then be limited only by that of the temperature measurements.

For most purposes, however, a comparatively rough compensation is all that is needed. With our apparatus as at first used there was a tendency, under ordinary working conditions, for the compensation to be incomplete, an unfrozen liquid showing a slow rise in temperature. This was corrected in a sufficiently effective manner by filing a small notch in the lower edge of the inner tube of the cooling device, thus permitting a very slight flow of brine at this point even when the ground surfaces were in contact.

We have put the method to a practical test in an extended series of determinations of the freezing points of water solutions. A Beckmann thermometer with a 5° scale was used throughout so that it was impossible to observe temperature differences of less than 0.001°, but within the limits of accuracy so imposed the results were very concordant and satisfactory. A few results obtained with two different solutions prepared from a carefully purified sample of cane sugar are given below by way of illustration:

Gms. sugar per 100 gms. H_2O	Observed f. p. lowering	Undercooling	Corrected f. p. lowering	Av.	Molecular lowering
4.274	0.239	1.81	0.234		
4.274	0.236	0.94	0.233	0.233	1.89
4.274	0.235	0.67	0.233		
8.521	0.487	1.10	0.481		
8.521	0.489	1.48	0.480		
8.521	0.485	0.88	0.480	0.480	1.93
8.521	1.27	1.27	0.480		

The thermometer used had been carefully compared with a similar one certified by the U. S. Bureau of Standards, and the observed freezing point lowerings as given in the second column of the table have been corrected for the errors of the thermometer. Those in the fourth column have been further corrected for the undercooling. Considering the lack of sensitiveness of the thermometer employed, and the fact that it was not kept in ice-water when not in use so as to avoid the effect of slow volume changes in the glass, the values found for the molecular lowering agree about as well as could be expected with the best available values for such solutions, as determined by Raoult and others.

[2]This correction is based on the degree of undercooling, the heat of fusion of the solvent, and the heat capacities (*a*) of the liquid, and (*b*) of the container, stirrer, thermometer, etc. In practice (*a*) is so large compared with (*b*) that a rough estimate of the latter is generally amply sufficient for the purpose.

Glucinum (Beryllium) and Its Alloys

BERYLLIUM, Be (discovered and so named by Wöhler, 1828), or glucinum, Gl, shares with niobium, Nb (isolated by Rose, 1844), or columbium, Cb, the unenviable distinction not only of being known under two names, but also of being designated by two symbols. To mark their disapproval of this treatment the two metals are still giving experimenters a great deal of trouble. Beryllium was supposed to resemble magnesium closely and to be an excellent conductor of electricity, until Fichter disproved the resemblance to magnesium in 1913, and showed that beryllium had a hardness of more than six, and that its conductivity was only one-twelfth that of copper; the name glucinum, used in England, had been adopted because the salts have a sweetish taste, differing in that respect strongly from the bitter magnesium salts. Investigating the metal and its alloys, G. Oesterheld (*Zeitschrift für Anorganische Chemie*, 97, pages 1 to 41, September, 1916) started, like Fichter, from the double fluoride $NaBe_2F_5$ (prepared from the carbonate), which he fuses and electrolyzes; the reduced metallic powder is compressed into cylinders which are melted in a vertical vacuum electric furnace at 1,300 deg. or 1,400 deg. C., when a metal of 99.5 per cent fuses out; this glucinum melts at 1,2785± deg. C. The difficulties of the preparation are that the first obtained powder retains sodium fluoride, and that it is very prone to cover itself with a film of oxide and to form a carbide in the presence of carbon monoxide both these compounds are formed. The heat of fusion of this metal, 277 cal. per gramme, is the highest known for metals, as was to be expected considering the high melting-point and the low atomic weight, nine; according to Compton's rule it should be higher still, 341 cal. Oesterheld prepares his alloys in tubes or crucibles of unglazed porcelain or of magnesia. Glucinum and aluminium, he finds, are miscible in the liquid state; they do not form any compounds, and the solid solutions contain eight per cent of Al maximum; the eutectic point is 644 deg. C. The alloys with copper are of a complex character. Two compounds, Cu_3Gl and $CuGl_2$, seem to be formed, in addition to four series of mixed crystals. The addition of glucinum to iron lowers the magnetic transformation-point of iron to 650 deg. C; there is one compound, probably $FeGl_2$, which melts between 1,400 deg. and 1,450 deg. C. Silver and glucinum do not combine and give only a limited range of solid solutions; with increasing glucinum percentage the ductility increases, while the hardness decreases. The study of alloys of magnesium and glucinum would have been particularly interesting; but magnesium boils at ordinary pressure below the melting-point of glucinum, and it is not known, therefore, whether the two metals are miscible or not.

Oesterheld, who works under Fichter at Göttingen, had only small bulks of metal to deal with, and in view of the high experimental difficulties further investigations will be necessary.—*Engineering*.

Methods of Estimating Distances at Sea in Fog or Thick Weather*

By Dr. J. Joly

THE method I have to propose is based generally on the differing velocities of signals in different media: (1) luminous or "wireless" signals (electro-magnetic disturbances) may be regarded as instantaneously propagated over the distances concerned; (2) submarine sounds—*e.g.*, bell-strokes under water—travel in the water at the rate of about 1,400 meters, or 4,700 feet per second; (3) aerial sounds move at the rate of about 330 meters, or 1,100 per feet second. Explosive signals travel somewhat faster in water and air.

Now, if simultaneously emitted signals are sent out in two differing media, the gain of the faster upon the slower traveling disturbance will be proportional to the distance traveled. Hence a ship picking up such simultaneously emitted signals can at once estimate her distance from the station from which they originated.

For instance, if a submarine bell be struck simultaneously with another bell which is above the surface of the sea, the relative displacement in time of the two signals will have amounted to 4.3 seconds at a distance of one nautical mile. At five miles it will be 21.5 seconds. The navigator, listening to the submarine sounds in the telephone, and provided with a seconds watch, easily estimates the interval separating these from the aerial sounds to one-half second. But this time lag corresponds to a distance of about 700 feet, or 117 fathoms. Closer readings of time are possible with many sound signals. Hence it is evident that the method proposed will give results enabling distances to be measured with an accuracy more than adequate to the objects in view.

A simplification in the nature of the observations required on board the ship, which possesses many advantages, may be secured by regulation of the signals. Thus we may arrange for the submarine signals to be sent out at intervals, which are definitely timed. There might be one bell-stroke every two seconds (as somtimes adopted at present), or even one bell-stroke per second. These signals are sent out in groups. There might be thirty successive strokes, followed by an interval of silence of half a minute. Simultaneously with the first stroke of each group the air signal is made. This convention is known to the mariner, and, when the first stroke of each group of bell-strokes is heard on the ship, he has only to count up the strokes till he hears the sound signal transmitted through the air in order to determine his distance from the signal station. Thus, if the bell-strokes are timed one per second, the air signal reaches a ship one mile away a very little after the fifth stroke of the bell. If the bell-strokes are timed every two seconds, the air signal coincides nearly with the third stroke of the bell coming about one-third second later. On this system the use of time-measurers on board ship is not required. The signals themselves tell the mariner his distance from the shore. The degree of accuracy secured is ample for the safety of his ship.

Wireless telegraphy is so generally at the disposal of ships at the present day that signals transmitted by its aid might well be used to supplement the submarine and air signals. There would be, then, signals simultaneously sent out in the three media. The special value of this would be that, in the event of particular atmospheric conditions interfering with the propagation of sound in the air, the approaching vessel can always judge her distance by the lag of the submarine signal on the aetherial. The sensitiveness here is about 1.2 seconds to the nautical mile. A knowledge of her distance from the signal station of less than one-half mile would be in most cases easily obtained. Again, in this case, vessels which had no auditory apparatus for submarine signals might determine distance by wireless and by air sounds. This combination secures the maximum sensitiveness, *i.e.*, about 5.5 seconds to the nautical mile. A ship at five miles from the signal station receives the aerial sound 27.5 seconds after the wireless signal. Where wireless was already installed at the signal station, the increased cost of sending out periodic signals with it would be insignificant contrasted with the advantages gained. It is, of course, open to choice whether the wireless signal should go out with every sound signal or only coincide with it periodically.

If the mariner can infer the direction in which he receives the sounds, whether in air or in water, and if he determines his distance from the signal station, he, of course, is able to define his position completely. Even if he cannot infer the direction from which the sounds reach him, but if he can lay down his own course upon the chart, his position is determined as one of two possible points by his distance from the signal station. If the sounds from two coastal signal stations are audible to him his position is defined even if he is, to start with, ignorant of his course upon the chart. For in the two distances he possesses the radii of two circles which are centered at the signal stations. Their intersection on the chart gives him his position. Finally there is the possible case (especially if submarine signals are not available) of his knowing neither the direction of the sounds nor his line of advance on the chart and hearing one signal station only. In this case his position is still determinable on the result of successive estimates of distance and a knowledge of his own compass course.

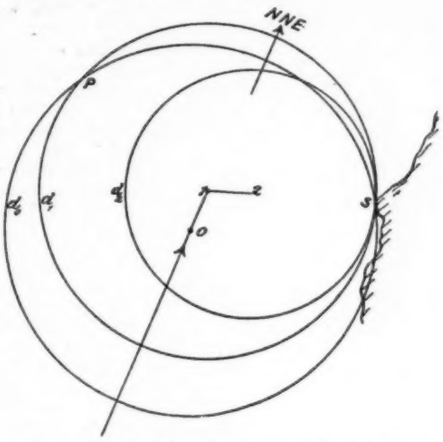


Diagram showing how direction as well as distance may be determined

As shown above the ship which is at the position O, and which is heading N.N.E., hears the signals from the coast and determines her distance as d_0 from the signal station. Setting out on paper the course direction N.N.E. and taking any point on this line as the position O, the mariner strikes a circle to the radius d_0. In five minutes say, he picks up another signal and obtains the distance d_1. He knows the new position of his ship, for he can estimate his own speed. This gives him the point marked 1. From this point a circle is struck to the radius d_1. This circle intersects the first circle on two points, P and S. These, in general, lie the one to port the other to starboard. If d_1 is greater than d_0 the two points lie aft; the ship is going away from the station. If d_1 is less than d_0 the conditions are reversed and she is nearing the station. The mariner knows that the signal station lies on one or other of the points P and S. He may decide between them by altering his course toward one of the points till he picks up a third signal. He is now at 2 and obtains the distance d_2. The circle to radius d_2 decides the question. In general the position is determined according as d_2 is greater or less than d_1. Having got the bearing of the signal station and its distance, his own position is fully known. There are two special cases when he can determine the bearing of the station without altering his course; when his ship is heading directly toward or away from the station. For in these cases the increase or diminution of the distance as given by any two signals will conform to the ship's run in the interval.

We have assumed above that the timing of the signals is not effected closer than to ½ second. As already stated this corresponds to an accuracy of 700 feet in the determination of distance. For all practical purposes this would suffice and, indeed, might be said to be needlessly precise. It is, however, worth pointing out that, even by unpractised observers, closer results would probably be obtained. For the error in effecting the readings is always of the same sign. What error there is must arise from operating the stop-watch after the exact moment of passage of the sound wave. Here the personal equation of the observer and any mechanical lag in the instrument are responsible for the delay. But both observations—that of starting and of stopping the seconds hand of the watch—are affected in the same manner by this source of error. It is the difference between the two readings we take for our calculation. The error, therefore, tends to be reduced or actually eliminated.

In high winds an error arises from the convection of the sound disturbance with the medium. This is not, under any ordinary conditions, important. It can in any case be allowed for. A wind blowing at the velocity of 40 knots, or say 60 feet per second, introduces an error which may be positive or negative in sign and may attain to about five per cent. This would be, in most cases, negligible. The variation of the velocity of sound with the temperature of the air is also capable of correction, but is so small as scarcely to call for consideration. Between 0° C. and 15° C. the velocity has been found to vary from about 330 meters per second to 340 meters per second. If neglected this introduces an error fo but three per cent. The velocity of sound in air or in water is greater than normal if the sound is explosive in origin. The special rates for explosions in air and in sea water have been recorded and if desired may, of course, be used.

For accurate timing of sound signals the nature of the sound must be abrupt, either upon starting or stopping, or of brief duration. Ordinary gun-fire signals are readable to a fraction of a second even at a distance of five or six miles. The effect of distance is to blur the definition, but it is to be remembered that at considerable distances this matters little. One second error in our readings introduces an error of one-quarter mile. In five miles this would, obviously, matter little.

*Abstract from an article by J. Joly, Sc.D., F.R.S., in the Proceedings of the Royal Society, London.

Generalized Relativity and Gravitation Theory

IN a number of papers published in several scientific journals during the past few years Einstein has developed a generalization of the original principle of relativity and has deduced certain results which seem to be of great importance. Since the mathematical analysis pertains to four-dimensional "space" and is extremely complex it will only be possible to give in this place a slight suggestion of these very original investigations.

The fundamental assumption consists in the "hypothesis of equivalence." Some idea of this conception may be obtained from the following considerations. Suppose an observer at rest in a gravitational field notices that all bodies fall with the same, constant acceleration. According to the non-generalized theory this state of affairs will be equivalent to the case where there is no gravitational field with the system of bodies at rest but with the observer moving in such a manner as to cause the bodies to appear to him to possess the same acceleration as they seemed to have when accelerated in the gravitational field. Because of the changes in the units of time and length in the two cases, the acceleration of the observer relative to the bodies in the second set of circumstances will not necessarily be equal numerically to the acceleration of the bodies in the first or gravitational field. Einstein's equivalence hypothesis consists in assuming that the two cases are not only equivalent for purely mechanical phenomena but that they are absolutely equivalent in all repsects—electrodynamically, etc. Another noteworthy feature of Einstein's theory is that it involves only the Gaussian constant and the velocity of light in the free ether. No other quantities having physical dimensions are introduced.

Attention may now be directed to certain practical deductions from the general theory. In the first place, it can be shown that a ray of light will be deflected by a large mass just as if the luminiferous vibrations were endowed with ordinary momentum. Accordingly, when light from a star passes close to the sun and then proceeds in such a manner as to be aimed at the center of the earth at the instant when the light strikes the observer's eye, the star will appear to be displaced from its true position on the celestial sphere for the reason that in passing through the sun's gravitational field the ray will be bent out of the line in which it had previously been moving. The theoretical angular displacement only amounts to 0″.83 which is probably too small to be observed.

A second special case relates to the solar spectrum. As a consequence of the fact that the rates at which synchronous clocks will run depends upon the potential of the gravitational field in which they are immersed, it follows that solar spectral lines should appear to an observer on the earth to be displaced toward longer wave-lengths. The magnitude of the shift is given by the approximate equation $(\nu_0 - \nu)/\nu_0 = 2 \times 10^{-6}$, where ν_0 denotes the frequency of the light vibrations on leaving the sun, and ν stands for the frequency on reaching the earth. Displacements of this magnitude and direction have been observed by L. E. Jewell and others, but, unfortunately for the present theory, it has not been possible to eliminate other causes which might have produced the effects observed; increase of pressure, for example.

The only quantity of appreciable size for which the theory seems to account perfectly is the secular motion of Mercury's periphelion. If this be true, and not a coincidence, then Einstein has overcome a difficulty which has completely baffled all astronomers. A certain angle associated with the orbit of Mercury has the values +118″.00 and +118″.58 according to observation and to Einstein's theory, respectively. The agreement is perfectly satisfactory since the probable error of the experimental datum is ±0″.43. On the other hand, according to Newcomb, the difference between observation and theory amounts to as much as +8″.48 (instead of −0″.58±0″.43) when the computations are based on the laws of Newtonian mechanics.—From a note in *The American Journal of Science*.

Rhubarb*

Notes on Its History, and the Use of Its Leaves as a Vegetable

"It is the interest of Mankind that all persons should be caution'd of advent'ring upon unknown herbs and plants to their prejudice." These words, written by John Ray more than two centuries ago, and quoted by his distinguished contemporary, John Evelyn, in his "Acetaria," are seasonable still, and, indeed, in view of the recent "advent'ring" with regard to rhubarb-leaves, have today a special significance and interest. Were our famous countryman of Stuart times living at this hour, it is quite conceivable that, great experimentalist as he was, and endowed with more than the usual share of the "interest of Mankind," he would have devoted himself with energy and skill to the solution of some of the problems that confront us now, and some pertinent remarks on the question of utilizing rhubarb-leaves as a vegetable would have been likely to appear over his signature in the columns of the daily newspapers. Had he in such circumstances recommended them, we can well imagine that his recommendation would have been accompanied by a warning similar to that quoted above, or more cogent, and printed in large clarendon capitals or italics.

An appeal has been made to history to supply an authority for consuming rhubarb-leaves now, and some prominence has been given to the statement which reposes in some books of considerable authenticity that they were used as a pot-herb in Queen Elizabeth's time. If they really were so used, and even with perfect safety, and were then "considered to be superior to spinach or beet," it is poor comfort to offer to those who in 1917 are suffering the tortures of poisoning arising as a consequence of eating them. That numerous cases of more or less serious illness, and at least one fatality, as reported within the last few weeks in the daily press, have followed the eating of rhubarb leaf-blades, is accepted as a fact which should leave no no doubt in one's mind that they form to many people an unwholesome and even a dangerous food.

In inquiring into the use of rhubarb, mainly with the view of getting evidence from the records of the past as to the use of its leaves as a vegetable, and what were the opinions held regarding such a practice by those who have gone before, some notes which here and there may contain fragments of interesting and useful information have been accumulated, and may be worth putting on record in a collected form.

It is not intended to go far into the botany of rhubarb. The vexed question of the source, or sources, of medicinal rhubarb has led to much controversy. That does not concern us here. The rhubarb used for culinary purposes today appears to have originated from more than one species. Some writers attribute its origin to *Rheum Rhaponticum*, Linn., and there seems no reason to doubt that it was this species that was first used in this country for culinary purposes, as well as being the first grown in England for its medicinal root. Moreover, it was the first species introduced into cultivation here, and from early times has been known as English rhubarb. Another species believed to be the parent of culinary rhubarb is *R. undulatum*, Linn., introduced in 1734, while *R. hybridum*, Murr., which, according to Aiton, was introduced in 1763 by Dr. John Hope, F.R.S., who had a garden at Upton, West Ham, is claimed by some authors as the original source of the common garden rhubarb of today. All three, and probably other species, are involved in its parentage. For many years it has been cultivated in many varieties differing in size and color of leaf-stalks, flavor and in degree of earliness.

Evelyn did not appear to know the rhubarb plant. He does not allude to it in his "Acetaria." Nor can we get any evidence from other writers of his time to support the reiterated statement that *Rheum Rhaponticum* was introduced in 1573, and our investigations induce us to say that whatever else flourished in this country in Queen Elizabeth's reign no species of Rheum had any chance at all, for none was in the gardens of her day. Whatever delights and good times the Elizabethans had, they owed nothing to a dish of stewed rhubarb or a rhubarb-tart, and whatever bad times—whatever pains they endured—could not be laid to the charge of rhubarb-leaves in any form. The delights arising from the former were reserved for a much later, if not more fortunate, generation, and the tortures arising from the latter for our more immediate forefathers in some degree, but chiefly for ourselves.

We have suggested that the statement that rhubarb-leaves were used as a pot-herb in Queen Elizabeth's time cannot be trusted. It is apparently based on a mistake which originated out of a confusion of terms John Gerard described and figured a certain plant in the first edition (1597) of his famous "Herball," under the name of *Hippolapathum sativum*, Patience, or Munkes (Monkes) Rubarbe, the last name "bicause as it should seeme some Monke or other have used the roote heereof in steede of Rubarbe." This. he says, "is an excellent holsome potherbe," but "it is not so pleasant to be eaten as either Beetes or Spinage." There is no doubt whatever that this plant is not a true rhubarb, but is a dock, and has been rightly referred by careful writers to *Rumex Patientia*, Linn., Herb Patience, a native of Southern Europe and the Orient. The name, "Monk's rhubarb," has also obtained currency in many works, including Syme's edition of "English Botany," for *Rumex alpinus*, Linn., a dock with large, roundish, radical leaves, found occasionally in this country, presumably as an escape from cultivation. This plant was known to Gerard, who included it in his "Herball" under the name of *Hippolapathum rotundifolium* (Bastarde Rubarbe), and he cultivated it in his garden in Holborn. Both these docks were evidently in gardens of the sixteenth century, and possibly long before, and were cultivated as pot-herbs, or the latter, according to Gerard, as a medicinal plant. Medicinal rhubarb was known to Gerard, but evidently only in the form of the dried root, which he figures. No evidence has been discovered to prove that any species of the true rhubarb (Rheum) was in cultivation in England before early in the seventeenth century, when John Parkinson, some time (probably not many years) before 1629, obtained a plant of what is now regarded as *Rheum Rhaponticum*, Linn. This he cultivated, and it is figured and described in the first edition of his "Paradisus Terrestris," 1629, under the name of *Rhaponticum verum seu potius Rhucarbarum verum*. Of it he wrote: "I have a kinde of round leafed Dock growing in my Garden, which was sent me from beyond Sea by a worthy gentleman, Mr. Dr. Matth. Lister. one of the Kings Physitians, with this title, *Rhaponticum verum*, and first grew with me, before it was ever seen or known elsewhere in England." After some reference to the character and medicinal properties of the roots, he continued: "The leaves have a fine acide taste. A syrrupe therefore made with the juice and sugar, cannot but be very effectual in dejected appetites, and hot fits of agues; as also to help to open obstruction of the liver, as divers have often tryed, and found availeable by experience."

By some curious blunder Monk's rhubarb has also been identified with *Rheum Rhaponticum*; hence in many works it is stated that this plant was introduced in 1573, apparently on no better evidence than is supplied by the fact that Tusser included the name "rhubarb" in his "Five Hundreth Points of Good Husbandry" of that date. In the edition of 1672 this name, without any qualification whatever, occurs in a list under the heading, "necessary herbs to grow in the Garden of Physick not rehearsed before." This "rhubarb" is probably *Rumex Patientia*, or *R. alpinus*—in "English Botany" it is represented as the latter. It is practically certain that it was not *Rheum Rhaponticum*.

It will be noticed that Parkinson refers to the fine acid taste of the leaves of the rhubarb which he cultivated. It is not clear whether he was alluding to the leaf-blade or leaf-stalk, but apparently he viewed this plant only as medicinal, and it seems impossible to determine the approximate date when rhubarb was first used for culinary purposes as we use it today. The practice of so using it was known to Philip Miller in 1752, for in the sixth edition of his "Gardeners' Dictionary" he wrote: "This sort (*Rheum Rhaponticum*) is frequently cultivated in the gardens, and is call'd English Rhubarb. The roots of this enter as an ingredient into several compound medicines; and of late years, the footstalks of the leaves have been used for making of tarts in the spring of the year, as these may be had before gooseberries are large enough for that purpose. These footstalks must have their outer skin peel'd off, otherwise they will be very stringy: when this is done, the pulpy part will bake very tender, and almost as clear as the apricot; and having an agreeable acid flavor, is by many persons esteemed for this purpose."

Rheum Rhaponticum has been cultivated in the neighborhood of Banbury, mainly for the sake of its root, since about the year 1777. W. Bigg, writing in 1846 (*Pharm. Journ.*, vol. vi., p. 75) on its cultivation there, said: "Of the leaves, I believe no use is now made. except the use common to all vegetable offal—manuring. The leaf-stalks are now very partially sold for the table. In former years, the sale of the leaf-stalks formed a part of the trade, but it can scarcely be said to do so now. Wine has been occasionally made of them, but not to any important extent. . . . The leaves were some years ago in demand (I have reason to think) for the adulteration of tobacco, or the manufacture of cigars, but are not at present."

It is stated in *Loudon's Gardeners' Magazine*, vol. vii., 1831, p. 369 that poor people in the neighborhood of Glasgow were in the habit of using rhubarb-leaves as a remedy for or for the relief of, rheumatism. Heated leaves were applied to the parts affected

If there was anything like a general appreciation of rhubarb as a substitute for fruit about the middle of the eighteenth century it must have declined so much in favor as to have been little used at the beginning of the nineteenth, for it is recorded that Mr. Joseph Myatt of Deptford about the year 1810, sent his two sons to the Borough Market with five bunches of rhubarb, and of these they succeeded in selling only three. But he persevered in his efforts to make a market for the vegetable, raised improved varieties, and before many years had elapsed rhubarb as a culinary plant was established in public favor. According to *Loudon's Gardeners' Magazine*, vol. iv., p. 245, at the beginning of June, 1828, the demand for rhubarb in the Newcastle-upon-Tyne market was so considerable that 100 sticks sold for 5*s*. In 1831 (*loc. cit.*, vol vii., p. 682) the culture of tart-rhubarb had increased so rapidly about Edinburgh that one grower for the market, who a few years before found great difficulty in selling forty or fifty dozens of bunches of stalks in a morning, sold from three to four hundred dozens of bunches. The common price of tart-rhubarb in the Edinburgh market at that time was 2*d* a bunch of a dozen stalks, while in Glasgow the same quantity was sold for 3*d*.

We are informed that Myatt obtained his first roots from Isaac Oldaker, gardener to Sir Joseph Banks, and Oldaker had brought them from St. Petersburg, having been gardener to the Emperor of Russia. They represented a finer and earlier kind than those previously cultivated in English gardens.

Several papers in the Transactions of the Horticultural Society of London show that in the second and third decades of last century a great deal of attention was paid to the forcing and blanching of rhubarb. In 1824 Mr. James Smith, gardener at Hopetoun House, was awarded the society's silver medal for devising a simple, effectual, and economical mode of forcing the plant. It appears that the method of blanching was discovered by accident in the Chelsea Physic Garden in 1815 (Trans. Hort. Soc. Lond., vol. ii., p. 258).

It was long ago realized that the use of rhubarb as food was attended with some risk to health. Lindley ("Vegetable Kingdom," 1846, p. 503) remarked that oxalic acid is copiously formed in both docks and rhubarbs, and that the latter also contain an abundance of nitric and malic acids. While these give an agreeable taste to the stalks of rhubarb when cooked, he regarded them as being ill-suited to the digestion of some persons. The "Penny Cyclopaedia," 1841, warned persons subject to calculous complaints against eating tarts made from rhubarb leaf-stalks, owing to the presence of oxalic acid, and that "the formation of the oxalate of lime, or mulberry calculus, may be the consequence of indulgence."

A note in the *Gardeners' Chronicle*, 1846, p. 5, by Alexander Forsyth, who was gardener to the Earl of Shrewsbury at Alton Towers, Staffordshire, has been recently referred to in newspapers as showing that rhubarb-leaves were in use about that time for culinary purposes. Forsyth wrote: "We have been in the habit of eating the leaves of the rhubarb-plant for many years, and seeing that the fruit-stalks of this vegetable were counted as waste, I thought it very likely that they were the better part of the plant, and I now find that the pouches of unopened flowers bear the same relation to the leaves of rhubarb that cauliflowers do to cabbage-leaves, and may be obtained in great abundance, and that at a time (April) when all kinds of vegetables are valuable." He refers to using the young inflorescence, which he called Rhaflower, "as a boiled vegetable, to be used like broccoli." The meaning of his statement about eating the leaves of rhubarb was not clear then, but in a subsequent note (*Gardeners' Chronicle*, 1847, p. 325) there is no doubt at all that by leaves he meant the leaf-stalks, and not the blades, for he wrote: "I have no experience in the eating of the leaves, and think them nauseous to the taste and unpleasant to the smell, and it seldom happens that any article is good for food when all the three senses of sight, taste, and smell reject it; it is not a good green color. I tasted them boiled, and they did not appear to me to have on

*This article was prepared for the *Kew Bulletin*, but as that publication was temporarily suspended on account of the war it appeared in *Nature* (London), from which journal it is reproduced here.

redeeming quality to keep them an instant from the dung-heap." In the latter note Forsyth again referred to eating the cooked flower-heads of rhubarb, and stated that he and others had done so without experiencing any ill-effects. But he directed attention to the fact that during the season (spring, 1847) there was a general complaint against the eating of the stalks of rhubarb-leaves, as violent relaxation had resulted. Another correspondent to the *Gardeners' Chronicle* (1847, p. 325) suggested that illness from eating rhubarb—apparently he meant the inflorescence—may have been due to the variety, and stated that a medical man whom he knew had a plant of rhubarb in his garden which was particularly early, and which, used in tarts, invariably caused illness in those who ate it, while other plants growing in the same bed, but which were a little later, were quite wholesome. The same effects had been observed for several years, until at length he destroyed the offending plant.

A reference to the *Gardeners' Chronicle* (1847, pp. 283, 341, 357) will show the varying results of eating the young inflorescence, producing no ill-effects in some cases and serious illness in others; and in the same journal (1847, p. 283) a case is recorded of a Chelsea woman who boiled rhubarb-leaves as a substitute for spinach, and all three of those who ate of the dish were attacked with sickness, one of them, a boy, being also afflicted with swellings about the mouth. An editorial comment on this runs as follows: "We are not aware of any similar instances of serious consequences following the use of rhubarb, but it is by no means surprising that a plant which forms so much oxalic acid should be unsafe, and we recommend the subject to serious chemical inquiry. It is quite conceivable that the leaves should contain some principle which the stalks are deficient in, as indeed is proved by the different manner in which the juice of the leaf-stalks and leaves is affected by the same reagents; but until there shall have been time for a careful inquiry into the organic products of these two parts we can only warn the public against employing for food any part of the rhubarb except that which experience shows to be harmless."

The Garden (1872, vol. i, p. 197) contains an extract from an American paper which shows that a woman residing between Oneida and Durhamville, New York, died from the effects of eating as greens the leaves of rhubarb, or pie-plant as it is known in the United States, but death taking place after three weeks of suffering. "The leaves are poisonous, and should never be eaten." concludes the paper's announcement of the fatality.

Judging from published statements (*Gardeners' Chronicle*, ser. 3, vol. xv, pp. 340, 353, 384, 400), there was a revival of interest in the question of eating rhubarb-leaves in 1899. One correspondent wrote (p. 384): "Rhubarb spinach has been for many years a favorite dish with us"; but the Secretary of the Massachusetts Horticultural Society communicated the following warning (p. 400): "The *Gardeners' Chronicle* for May 27th is at hand this morning and the note on 'Rhubarb-leaves as a Vegetable' prompts me to say to you that instances have been known here where their use as 'greens' has caused fatal results owing to the excess of oxalic acid A horticultural friend told me many years ago that he had raised many seedlings, some of which (I assume that the usual part was cooked in the usual way) caused vomiting as certainly as ipecacuanha."

A curious case is reported in the *Pharmaceutical Journal* (1901, vol. lxvi, p. 639) as follows: "At an inquest held at Ashstead on Friday, May 3d, concerning the death of John Lintott (thirty-nine), a scaffolder, it was stated that on the previous Monday deceased complained of violent pains and a doctor prescribed for him having found that he was suffering from a gastric attack After the doctor left the patient some cooked rhubarb-leaves were given to him as medicine, it being stated that the leaves were used as a vegetable in parts of Hampshire. The man died next day, and the doctor expressed the opinion that death was due to excessive vomiting, causing exhaustion, produced by eating rhubarb-leaves. The coroner expressed surprise at hearing that stewed rhubarb-leaves were used as a medicine or as a vegetable. A verdict was returned of 'Accidental death, caused by eating rhubarb-leaves.'"

In 1911, vol. lxxxvi, p. 8, the same jounral contains the following, extracted from the *British Medical Journal* of December 31st, 1910: "The author [Dr. W. E. Burton] mentions two cases of rhubarb-poisoning to which he was called, the symptoms being similar in each case, and refers to the death from the use of rhubarb which was the subject of a coroner's inquest at Catford some weeks since. Rhubarb, although rightly regarded as a wholesome food and an excellent substitute for fruit, does not agree with everyone. It is possible that the presence of oxalates in the urine and the severe intestinal irritation indicate oxalic acid as being one of the agents responsible for the toxic action. Oxalic acid and oxalates, chrysophan, chrysophanic acid, and phaoretin are all found in rhubarb-root, and are of an irritating nature."

In a discussion on rhubarb-wine (*Gardeners' Chronicle*, 1853, p. 406) the observations of one writer seem to have especial interest as a possible explanation of the cause of the variable effects produced by eating rhubarb: "However good the wine made from rhubarb may be, I take the liberty of advising your readers not to drink it. It is well known that the acidity of rhubarb-stalks is owing to the presence of an acid salt—the binoxalate of potash—a combination of the poison oxalic acid and the alkali potash. This salt does not exist in sufficient quantity in the rhubarb-stalks to produce its poisonous effects, and the same may be said of the wine. But there is another danger attending its use in the form of wine which ought not to be overlooked. All hard water contains lime, and when mixed with the juice of the rhubarb-stalks the binoxalate of potash is decomposed and an oxalate of lime is formed. Now this oxalate of lime is the constituent principle of the mulberry calculus, and there is a peculiar condition of the human body known to medical men as the oxalic diathesis, which depends upon the presence of this oxalate of lime in the blood (I use the word blood for obvious reasons). This oxalic diathesis has been proved by Dr. Golding Bird to be much more common than it was supposed before this gentleman brought the microscope to assist him in his pathological researches. Such being the case, it is obvious that any article of common use which contains this oxalate of lime, or even the oxalic acid or its salts, must be more or less injurious to health, more particularly to those in whom there exists a predisposition to assume the oxalic diathesis. It must be borne in mind that oxalic acid is formed in the human body by the decomposition of sugar, urea, etc., and the diathesis is not uncommon from this cause If it is thus easily produced indirectly, *a fortiori* it is still more likely to arise from the direct means of rhubarb-wine. Therefore I say to your readers, eschew the doubtless very agreeable beverage which has entered, through the medium of your columns into competition with genuine 'Sillery mousseaux.'"

The eminent physician and chemist, Dr. William Prout, F.R.S. (1785-1850), regarded rhubarb as likely to be a dangerous food owing to the large amount of oxalic acid present in the leaf-stalks. Having analyzed wine made from the stalks, he considered it a most pernicious drink, and that its frequent use was likely to produce stone in the bladder. He expressed the opinion that an Act of Parliament ought to be passed, if necessary, to prevent the sale of so dangerous a poison (*Gardeners' Chronicle*, 1853 p. 438).

There is possibly something in the suggestion that the chemical composition of rhubarb varies to some extent according to the variety and also according to the soil on which it is grown. A writer in the *Gardeners Chronicle* (1853, p. 357) stated that the amount of water present was less when the plants were grown on poorer soil, while the acid principle was more abundant.

Mr. Edward Solly, F.R.S., published in the Transactions of the Horticultural Society of London, ser. 2, vol. iii., 1848. pp 35-92, the results of his experiments on the inorganic constituents of plants. Among the numerous plants on which he experimented were several rhubarbs. In the case of each of these he gives the respective amounts of water, organic matter, and inorganic matter found both in leaves and leaf-stalks. In every case, as he shows by figures, there was considerably less water present in the leaves than in the leaf-stalks, but in most cases almost double, in a few more than double, the amount, always very considerably more of organic and inorganic matter was present in the former. It is therefore natural to assume from the results of his investigations that oxalic acid, or whatever is deleterious in the rhubarb-plant, is present in greater proportions in the leaf-blade than in the leaf-stalk.

It may be left to the discretion of those who chance to read this article to decide whether or not it is advisable to eat cooked rhubarb-leaves or rhubarb in any form. For at least a century the consumption every year of the leaf-stalks as a substitute for fruit has been enormous. It is well known to be usually a wholesome, and certainly a useful, food. Compared with its extensive use, the cases of illness charged against it may be regarded as negligible. The inflorescence has also been tried, but evidently not very much, and with diverse results. The consumption of the leaf-blades has apparently never been general or considerable, by no means comparable with that of the leaf-stalks, but the baneful effects of doing so are relatively so marked that it may be said decisively that rhubarb leaf-blades cannot be recommended for general use as a food. While experiments in such matters are often necessary, and, if attended with caution, are desirable, carelessness in recommending them or in putting them into practice may place one in a less enviable position than those of whom it has been said, "Happy from such conceal'd, if still do lie, of roots and herbs the unwholsom luxury"; and the injudicious experiment in eating insufficiently tested articles of food may lead one to "discover their malignity in dangerous and dreadful symptoms."

Suction Turbulence*

A Vital Factor in the Working of all Petrol Engines

It is probably unknown to the majority of our readers that the British Association has for a number of years been working, through a committee of scientists and engineers, upon the subject of gaseous explosions. Undoubtedly the three most prominently connected with this research have been Dr. Dugald Clerk, Prof. Betram Hopkinson, and Dr. W. Watson. Other famous scientists have assisted materially, but it is to these three that we owe most, and we believe every member of the committee would be unanimous in agreeing that the one who has specialized most deeply and for the longest time on the subject is Dr. Dugald Clerk, the author of that well-known standard work, "The Gas, Petrol, and Oil Engine." This, however, is but natural, as the other scientists have had a number of subjects to deal with, while in the main, right away from the very early days of the gas engine, Dr. Dugald Clerk has specialized intensely upon it.

THE THEORY STATED

We do not attempt to summarize even briefly the work of the Gaseous Explosion Committee, but rather to deal with one of its most interesting researches into what, in our opinion, is perhaps the most remarkable of the sequence of events which take place so rapidly in the cylinder of a motor car or any other internal combustion engine. The high frequency explosion engine is, of course, used more widely on motor vehicles than any other, and at first sight one would expect little in common between the 3,000 r.p.m. of the little motor cycle engine and the majestic 150 to 170 r.p.m. of the great gas engine. Yet as Kipling said of the colonel's wife and the sergeant's wife, "They are sisters under their skins."

In Dr. Clerk's own words.

"The mixture of gas and air which is subsequently to be exploded enters the engine through the valves or ports at a high velocity, so that the gas within the cylinder is in a state of eddying or turbulent motion. This motion gradually dies away after the valves are closed, but will persist for some time during the compression stroke, so that at the moment of explosion there may be still a great deal of turbulence. In consequence of this motion of the explosive mixture the convection of heat will go on more rapidly, and what may be called the 'effective conductivity' of the gas will be increased."

EXPERIMENTAL PROOF

It is difficult to conceive of anything clearer than this, but possibly motorists who have not looked into the matter closely will understand it even better if we very broadly paraphrase the last sentence as meaning that the turbulence or commotion of the gas induced by its high velocity into the cylinder causes the whole mass to inflame very much more rapidly than if it were in a state of quiescence or even of moderately rapid motion. The motion is rapid and violent, as the word turbulence denotes.

Undoubtedly the finest exposition on the subject of turbulence as a whole is to be found in Dr. Clerk's paper read before the Royal Institution some two years ago, and we make no excuse for quoting extensively from that section which deals with suction turbulence. In the previous part of the paper Dr. Clerk dealt very fully with the results of experiments made at Cambridge by Prof. Bertram Hopkinson on explosions in a closed vessel, in which the same mixtures as those used in a gas engine were employed, but the explosive charge was in a state of quiescence to begin with, and the conditions were then altered by setting up what one may call artificial turbulence by running a fan inside the closed vessel, which acted more or less as a churn, and gave that degree of swirling and eddying which is so absolutely necessary for rapid combustion of the fuel, whether it be a mixture of coal gas and air or of petrol vapor and air.

EXPLOSIONS IN CLOSED VESSELS AND ENGINES

Dr. Clerk said it had long been known that the times of explosion in closed vessel experiments were much longer than the times found in engine cylinders with moving pistons. Indeed, Prof. Hopkinson's experiments with a closed vessel had shown that with an engine

*From the *Autocar*.

cylinder approximating to the same dimensions it would be impossible to work it commercially, as the time of explosions would be as long as .26 second. Therefore the single revolution of such an engine would occupy 2.3 seconds, so that the engine could not revolve even at as low a rate as 30 r.p.m. and still ignite the gases in time to produce effective results. As long ago as 1882 Dr. Clerk had pointed out that mechanical disturbance of the explosive mixture in the cylinder was necessary in order to secure the maximum pressure in the time required for running an engine effectively, but at that date the full significance of suction turbulence was not appreciated, though the theory than held by Dr. Otto and others of the stratification of gases was hopelessly wrong.

FLAME PROPAGATION

The examination of indicator diagrams from gas engines and petrol engines had shown that the rate of flame propagation varies in different engines from 35 feet to 100 feet per second. In a gas engine using town gas and running at 160 r.p.m., the time of explosion was $\frac{1}{30}$th of a second, which gives a flame velocity of 85.5 feet per second, amd in this particular engine the inlet velocity of the charge through the valve throat was 160 feet per second. We want this velocity of the inlet charge to be borne in mind. It will be seen that it is at the rate of 9,600 feet a minute; in other words, assuming a piston speed of 1,000 feet per minute, it is nearly ten times the piston speed, and the importance of this fact will be realized.

To take a typical petrol engine running at high velocity examined by Dr. Watson, it was found that the explosion period occupied $\frac{1}{300}$th of a second, this giving a flame velocity of approximately 90 feet per second. It will be noticed, despite the rapidity of the explosion, that, owing to the very much smaller diameter of the engine as compared with the great gas engine, the actual flame velocities are almost the same —85.5 feet per second for the gas engine and 90 feet per second for the petrol engine.

While it was so well recognized that the flame velocity was higher in engine cylinders than in closed vessels, and mechanical disturbance was assumed to be the cause, yet many years elapsed before the matter was fully investigated and explained.

SUCCESSIVE COMPRESSIONS AND EXPANSIONS

Indeed, it was not until 1912 that simultaneous experiments were made by Dr. Clerk and Professor Hopkinson by different methods to determine the nature of the mechanical disturbance causing this increase in the explosion rate. Dr. Clerk's experiments were made with a gas engine, and it was so arranged that a charge could be taken into the cylinder, compressed and expanded for any desired number of times without ignition, and then ignited under compression when the desired number of alternate compressions and expansions had been completed. The object of this experiment was to compare the rate of inflammation immediately after ignition and compression under full load with the engine hot and in the ordinary manner of working, with the rate of inflammation after the charge had been compressed and expanded several times before ignition.

RAPIDITY OF IGNITION

The idea to be substantiated or disproved was that turbulence was caused within the cylinder by admission of the charge at a high velocity through the inlet valve, and that this turbulence persisted during the compression stroke, so that in the ordinary running of the engine the ignition flame was projected into a mass of rapidly-moving gas, resulting in the flame velocity being much more rapid than if the charge had been still. The alternate compression and expansion before ignition was intended to allow the turbulence to subside, so that ignition started in a comparatively still mass of gaseous mixture instead of in a rapidly-moving one. The experiments proved that inflammation at the end of the third compression stroke took two and a half times, as long as if the ignition occurred immediately after one compression stroke, as in ordinary working, and show clearly that the velocity of flame through the mass of gas in the working cylinder depends very largely on the residual turbulence, due in the first instance to the charge velocity at entering. From this it would be deduced that the higher the charge velocity the greater the speed of ignition, and experiments show that this is the case. Where the charge velocity through the valve is high the rate of ignition is rapid, and in petrol engines, where the charge velocity sometimes rises as high as 150 feet per second, the flame velocity approaches 90 feet per second.

PISTON TURBULENCE NEGLIGIBLE

Professor Hopkinson's simultaneous experiments were those to which we have already referred, and consisted in using an agitator or fan inside the closed vessel in which the mixture was exploded. Without going into details it will suffice to say that these experiments entirely substantiated the engine experiments, as when the fan was run at a suitable speed the rate of explosion could be accelerated, as it was in the gas engine cylinder, by the turbulence. We may say that these experiments had certain other objects in view in regard to other aspects of gaseous explosions, but we purposely omit reference to them so as to keep down to the one aspect of the subject.

What we think will most puzzle the average reader is that Dr. Clerk should regard the alternate compressing and expanding of the charge as little better than leaving the gas at rest. He talks of residual turbulence which does not die out on the compression stroke, where the average man would imagine that the compression stroke would keep the gas in a very high state of excitation indeed. This is a very interesting point, and Dr. Clerk has been good enough to explain it very clearly. For the sake of distinction we call this expanding and compressing of the gas by the piston "piston turbulence," and, accepting the term, Dr. Clerk points out that the velocity of the gases through the inlet ports varies, as shown by his experiments, between 100 and 160 feet per second. The piston velocity in the petrol engine is only in the order of 20 feet per second. This gives 1,200 feet a minute. Of course, engines may run, and do run, considerably faster than this and also slower, but the fact remains that the velocity of suction is from five to eight times the velocity of the piston. With ordinary shaped compression space the piston turbulence is practically negligible, whereas the churning up due to the high velocity of the inlet is of such a violent order that it cannot die down during one compression stroke. When it is realized that the energy of motion due to a velocity of 20 feet per second (piston speed) is about $\frac{1}{25}$ of that due to 100 feet per second (suction speed), it will at once be seen why suction turbulence is the controlling factor and not the relatively small displacement due to the piston movement.

SUCTION VELOCITY THE VITAL FACTOR

Suction velocity, that is the velocity of the mixture current through the inlet port, varies with the speed of the piston. Inferior though the piston speed is, obviously it is the provocative cause of suction velocity, which also depends upon the ratio between the mean area of the inlet passage and the piston area: the smaller the inlet orifice and the bigger the piston, the greater the velocity with which the mixture will rush through the port. This last point is quite an interesting one, as we have heard it said before now, even when engineers have been discussing high efficiency engines, that the ideal inlet valve, in regard to getting a full charge, would be a valve of the same size as the piston.

This is all very well so far as it goes, but it must be borne in mind that it is not merely a question of the size of the valve. If we had a valve so large that there was a poor suction effect, the velocity of the inlet current would be reduced so enormously that it would be necessary to adopt some meas of forcible feeding, such as an air compressor; and, if our memory does not deceive us, we believe it was Dr. Clerk who pointed this out in connection with certain high efficiency engines which showed quite clearly that, beyond certain limits, increase in valve sizes did not result in satisfactory running until an enormously high piston speed was attained, so that the engines were of no use except over very high speeds. Incidentally this is another argument for the four valve per cylinder idea, though, of course, it presupposes that the valves are not timed to open simultaneously unless desired. However, this is going away from the main subject, and we need not pursue it further at the moment.

In conclusion, we should like to express our indebtedness to Dr. Dugald Clerk for the trouble he has taken in enabling us to make this matter clear to our readers.

A New Tantalum Alloy

ASIDE from their beauty, which gives them value in the arts, gold and platinum owe their usefulness industrially largely to their resistance to oxidation and the ease with which they can be worked because of their malleability. Tantalum, a metal which has been known for a century, is likewise resistant to oxygen and to acids, at least at practical temperatures, but it was long unused because of the difficulty of melting it or causing it to amalgamate with other metals. A special technique is employed in making the filaments used in tantalum lamps. For this purpose it is reduced to a fine powder and mixed with other powdered metals. The mixture is then subjected to strong pressure to form it into a compact mass, and afterwards fused in an electric furnace.

By using the same method to combine it with small proportions of gold and copper a homogeneous alloy has now been obtained, very similar to gold in color, and with properties which it is expected will enable it to take the place of other precious alloys in all the industrial uses for which they have hitherto been considered indispensable. The new alloy is very ductile, very resistant, very flexible, is as non-oxidizable as pure tantalum under the same conditions, and can be produced at a very reasonable price.

SCIENTIFIC AMERICAN SUPPLEMENT

Founded 1876

NEW YORK, SATURDAY, AUGUST 11, 1917

Published weekly by Munn & Company, Incorporated Charles Allen Munn, President; Frederick Converse Beach, Secretary; Orson D. Munn, Treasurer, all at 233 Broadway, New York

Entered at Post Office of New York, N. Y., as Second Class Matter

Copyright 1917 by Munn & Co., Inc.

The Scientific American Publications

Scientific American Supplement (established 1876) per year $5.00
Scientific American (established 1845) 4.00
The combined subscription rates and rates to foreign countries, including Canada, will be furnished upon application. Remit by postal or express money order, bank draft or check.

Munn & Co., Inc., 233 Broadway, New York

The purpose of the Supplement is to publish the more important announcements of distinguished technologists, to digest significant articles that appear in European publications, and altogether to reflect the most advanced thought in science and industry throughout the world.

Back Numbers of the Scientific American Supplement

SUPPLEMENTS bearing a date earlier than January 1st, 1916, can be supplied by the H. H. Wilson Company, 39 Mamaroneck Avenue, White Plains, N. Y. Please order such back numbers from the Wilson Company. Supplements for January 1st, 1916, and subsequent issues can be supplied at 10 cents each by Munn & Co., Inc., 233 Broadway, New York.

WE wish to call attention to the fact that we are in a position to render competent services in every branch of patent or trade-mark work. Our staff is composed of mechanical, electrical and chemical experts, thoroughly trained to prepare and prosecute all patent applications, irrespective of the complex nature of the subject matter involved, or of the specialized, technical, or scientific knowledge required therefor.

We also have associates throughout the world, who assist in the prosecution of patent and trade-mark applications filed in all countries foreign to the United States.

Branch Office:
625 F Street, N. W.,
Washington, D. C.

MUNN & Co.,
Patent Solicitors,
233 Broadway,
New York, N. Y.

Table of Contents

PAGE

The Nature of Matter—I.—By J. C. Whitehorn 92
Muscular Power and Endurance of the Human Body.—By Armin Elmendorf.—12 illustrations 93
Explorations in the Hawaiian Islands 95
Potato Flake Process 96
Weird Diseases of Africa.—By Walter B. James 97
Alcohols from Acetylene 97
Observations on the Friedel-Crafts Reaction 97
The Effect of Weather on Forest Fires.—By Andrew H. Palmer.—4 illustrations 98
The Electrical Properties of Gases—V.—By Sir J. J. Thomson 99
Aircraft Testing 91
An Apparatus for Determining Freezing Point Lowering.—By R. G. VanName and W. G. Brown.—2 illustrations 93
Glucinum (Beryllium) and its Alloys 93
Methods of Estimating Distances at Sea in Fog or Thick Weather.—By Dr. J. Joly.—1 illustration 93
Generalized Relativity and Gravitation Theory 94
Rhubarb 95
Suction Turbulence 95
A New Tantalum Alloy 96